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THE "TOTAL VALUE CRITERION": A NEW PRINCIPLE IN INDEX NUMBER CONSTRUCTION[1]

By Irving Fisher, *Yale University*

INTRODUCTION

In constructing an index number by which to measure the change of prices between two dates there are, in general, four chief problems:

(1) To decide on the exact *field* of items to be covered, implying specifically:
   (a) A group of *prices* (p's) to be indexed, and
   (b) A group of associated *quantities* (q's) to be used in forming weights expressing the relative importance of the respective items in the included field;
(2) To select, from this whole field of items, the best *samples* to represent it;
(3) To select the best *formula* to be used;
(4) To select the best *base*, or system of bases.

These are, in short, the problems of field, sampling, formula, and basing.

The present study is confined to the problem of sampling. The last problem, that of basing, will be the subject of a later study. No particular basing system will be favored in this article. The indexes here studied are individual links, month-to-month, each considered by itself without relation to the question of how best to form a time series of indexes.

The next to the last problem, that of formula, was the main subject of my *The Making of Index Numbers* and so need not be treated at length here. The best formula, according to my studies, is the "ideal" formula.[2]

[1] An expansion of one section of a paper read at the eighty-eighth annual meeting of the American Statistical Association, St. Louis, December, 1926.

[2] See *The Making of Index Numbers*, 3rd edition. (Pollak Foundation for Economic Research, Newton, Massachusetts), p. 220.

It is remarkable that the very worst formulae have been used for stock market indexes in spite of the

The first problem, that of delimiting the field, is one the solution of which depends eminently on the purpose in view. The purpose is often somewhat vague and it is always a difficult and somewhat arbitrary matter to decide what field we really want to include. In this article, however, it is assumed that the field-specifications have been decided upon. For the illustrative material used in this article, the specifications are as follows:

The *field* includes all industrial common stocks listed on the New York Stock Exchange (thus excluding all Railway and Public Utility stocks and all preferred stocks).

(a) The *prices* are average monthly prices.[1]

(b) The *quantities* are the numbers of shares sold, of the respective stocks, at the above specified prices.

Of course, it is quite possible to settle on somewhat different specifications. We might, for instance, with precisely the same group of stocks and prices, choose, as the associated quantities, the *number of shares outstanding;* this is, in fact, done by the Standard Statistics Company. Either method (shares sold or shares outstanding) is legitimate, according to one's viewpoint and purpose. The same formula applies in either case. But the quantities (the q's in the formula) have different meanings in the two cases.[2] Each affords, through the ideal formula, two associated index numbers, one of prices and the other of quantities. In the present case these two indexes are: (1) an index of industrial common stock prices, their relative importance being taken as dependent on their activity in the New York Stock Market; and (2) an index of their activity or quantities sold (their importance being conversely specified).[3]

There remains, of our four problems, only the second, sampling, which is the sole subject of the present article.

---

fact that fuller data are available for such indexes than for commodity indexes or for any other sort of indexes, so that the very best formula could readily have been applied. In some cases, in fact, the indexes actually in use do not even constitute true indexes (an average of price relatives) at all.

[1] The "average" price of any individual stock is taken as the simple mean of the monthly high, low, first, and last, quotations. In case of a stock dividend, etc., more accurate data and a suitably corrective procedure are used.

[2] It will be noted that Problem (3), the choice of a formula, and Problem (1), the choice of the field (both p's, price quotations, and q's, their associated quantities to go into that formula) are totally distinct problems. Many confuse (3), the purely formal question of the formula, with its concrete content (1)—what set of p's and q's are to be put into it.

[3] I have thus purposely escaped the need of debating what is a question of practical importance. I need merely add that weighting according to the number of shares outstanding, actually used by the Standard Statistics Company, seems to represent an index of the prices of stocks in existence rather than one of stocks traded in. Its correlative quantity index would register the changes in the quantities of such stocks in existence, a magnitude quite distinct from an activity index. The price index correlative to an activity index is the one here chosen for study and seems to be the one for which the public is groping when using the ordinary crude index, the average price of a fixed list of active stocks. It might be called a speculator's index as against the index of the Standard Statistics Company which might be called an investor's index.

For the stock market good sampling is more important than a good formula. Poor sampling with the best formula will give a worse index than good sampling with the worst formula.

Hitherto only very general considerations and very indefinite guides for sampling have been offered.

One such possible guide has already been suggested.[1] I am here presenting a new, and more definite, guide.

THE THREE TOTAL MARKET CRITERIA

This guide is the "Total Value Criterion." It is somewhat analogous to what is described in *The Making of Index Numbers* as "The Factor Reversal Test." The Factor Reversal Test prescribes that the index of prices multiplied by the index of quantities should give the true index of values (of the samples used). This test is one of the two chief tests for securing the best *formula*.

For securing the best *sampling* the analogous Total Value Criterion is our guide. It prescribes that our samples are to be so chosen that their *price index* multiplied by their *quantity index* shall give the *true value index* for the *whole field* represented by those samples.

The necessity of such a criterion was forced on my own attention in a study of stock market indexes. Without it, a stock market index of prices, and even more, a stock market index of quantities is apt to wander surprisingly far afield.[2]

Since no sample can be more typical of the total market than the total market itself, we may consider indexes for the total market as the final arbiter of stock market indexes—for prices and quantities as well as for values. Any smaller sampling may then be judged by reference to the corresponding total market indexes.

I shall here go to the trouble of calculating all three indexes (not only that of value but also those of prices and quantities) for the total market. While such a calculation is, of course, far too laborious for practical current index making, it will here be made for the purpose of checking the reliability of the results obtainable from sampling. If, in such a laboratory calculation, we find a set of samples which can be relied upon accurately to represent the total market not only for value indexes but for indexes of prices and quantities, we can, apparently, in future, calculate the price and quantity indexes merely from such sam-

[1] *The Making of Index Numbers*, p. 335.

[2] In fact the problem of a good index of quantities in the stock market, that is, an index of stock market "activity," has hitherto scarcely been attacked at all; the only index hitherto employed as an index of activity or quantities is a spurious index, the familiar "number of shares sold"—a conglomerate sum of heterogeneous elements, with "haphazard weighting." The Total Value Criterion entails a rigorous accountant's check-up on the product of the price and quantity indexes.

ples, only using the total market for computing value indexes as a check on the value indexes computed from the samples, and without calculating the separate total market indexes for prices and quantities. This will afford a great saving of time and labor.

Table I gives the values traded in dollars and the three indexes (the

TABLE I

TOTAL MARKET

INDUSTRIAL COMMON STOCKS

Months*	Value Traded	Monthly Index Links		
	$ (000,000) (1)	Value (2)	Price (3)	Quantity (4)
1925				
Jan.	1,931			
Feb.	1,386	84.4	103.1	81.9
Mar.	1,767	106.2	98.3	108.1
Apr.	1,190	70.3	99.1	70.9
May	1,393	117.1	108.5	107.9
June	1,214	83.5	103.6	80.6
July	1,457	117.6	106.9	110.0
Aug.	1,438	102.9	103.7	99.2
Sept.	1,807	128.4	105.2	122.0
Oct.	2,857	154.8	110.9	139.6
Nov.	2,982	116.8	104.1	112.3
Dec.	2,257	67.6	100.4	67.4
1926				
Jan.	1,920	88.8	103.2	86.1
Feb.	1,973	115.6	99.4	116.3
Mar.	2,664	108.0	90.9	118.8
Apr.	1,597	65.2	94.1	69.3
May	1,141	74.7	99.6	75.0
June	1,611	129.4	105.9	122.2
July	2,233	144.5	109.7	131.8
Aug.	2,784	119.5	108.3	110.4
Sept.	2,389	91.5	102.2	89.5
Oct.	2,659	111.3	98.9	112.6
Nov.	1,924	74.0	99.2	74.6
Dec.	2,548	119.0	104.4	113.9

* Values traded are given for each month. The index links shown opposite each month are calculated by dividing the value, price and quantity for that month by the value, price and quantity for the preceding month.

value index, and the indexes of prices and quantities) for the total market of industrial common stocks on the New York Stock Exchange, month to month, for 1925–26.

Chart I consists of graphs corresponding to Table I.

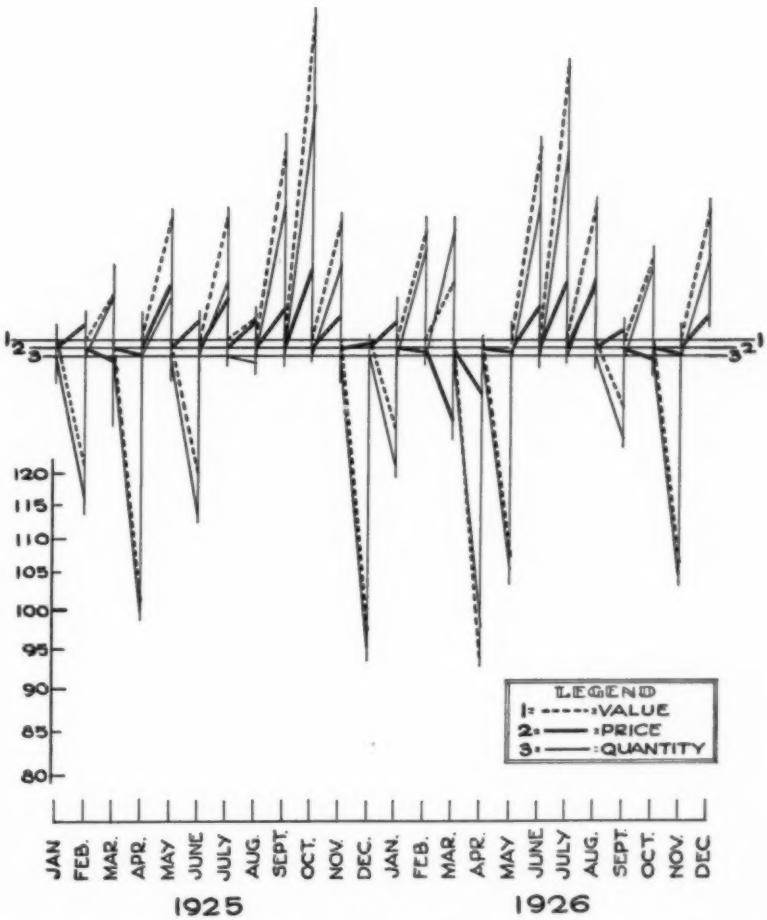
It would seem that those indexes represent accurately the move-

ments of the New York industrial stock market;[1] for they are calculated not from mere samples of that market but from the whole market itself. If this be accepted, our problem of sampling is simply to find samples

CHART I

TOTAL MARKET, INDUSTRIAL COMMON STOCKS

MONTHLY INDEX-LINKS; VALUE, PRICE, AND QUANTITY



[1] The only other total market indexes hitherto attempted are: (1), for prices, the average price of all stocks listed on the Stock Exchange, worked out by Mr. James Edward Meeker, the economist of the Exchange; and (2), for quantities, the total number of shares sold, published daily in most reports of the stock market.

These differ widely from the Total Market Criteria for prices and quantities. The chief reason for the wide deviation of the first is that Meeker's index is not a true index at all. It is an average price (not an average of price relatives). The result is, for instance, that his index is pulled down by every split-up

such that their value index, price index, and quantity index shall agree respectively with the above value, price, and quantity indexes for the whole market, which may be called the Total Market *Value* Criterion, the Total Market *Price* Criterion, and the Total Market *Quantity* Criterion. Our sampling may be said to be perfect if the value, price, and quantity indexes calculated from that sampling coincide absolutely with the above value, price, and quantity indexes, or criteria, calculated from the whole market. To the extent that they do not so coincide, the sampling is imperfect.

Evidently we might have coincidence in one of the three cases but not in the other two. Our samples might conceivably yield a value index agreeing with the Total Value Criterion and yet not yield price and quantity indexes agreeing with the price and quantity criteria. We do know, however, from the properties of the ideal formula[1] that if the value index computed from the samples is *not* true to the Total Market Value Criterion then the other indexes constructed from the samples, namely the price and quantity indexes, must likewise be untrue—one or both—to their respective Total Market Criteria.

While, therefore, the Total Value Criterion may not, of itself, necessarily insure a good sampling, any failure to conform to that criterion always insures a bad sampling, spoiling either the price index or the quantity index or both.[2]

In that case, either the price index or the quantity index, or, more probably, both, must be unrepresentative of the total market. Thus

---

of stocks and by every new listing when, as often happens, the new stock starts at a figure below the previous average.

Of course Meeker's total market is not entirely comparable with the total *industrial* market here considered, which omits Railway, Utility and all preferred stocks. For the same reason the total number of shares sold is not comparable with the total number of *industrial* shares sold. To secure better comparability I have therefore recalculated both these pseudo-indexes so as to make them relate to industrial stocks only. Even as so recalculated, these indexes differ widely from the Total Market Criteria for prices and quantities, each deviating therefrom equally and oppositely. The results are given in Chart V (price indexes) and Chart VI (quantity indexes).

[1] We know that the ideal formula satisfies the Factor Reversal Test. According to this $P_g Q_g \equiv V_g$ (where P_g signifies the index of prices; Q_g, that of quantities; and V_g, that of values—all three calculated from the samples) and $P_t Q_t = V_t$ (where the symbols have the corresponding significance for the total market). Evidently, if V_g is not equal to V_t then either P_g is unequal to P_t or Q_g to Q_t, or both. But if V_g and V_t agree then either the P's agree and the Q's agree or the disagreement of the former exactly offsets that of the latter.

[2] In this respect the criterion is like the two Reversal Tests investigated in *The Making of Index Numbers*. If the Time Reversal Test is not satisfied we know that either the forward index or the backward index is erroneous, or both. But the test cannot locate the error. Nor, if the test is satisfied, can we be sure that no error exists.

In a somewhat analogous way the accountant checks up two tentative figures against each other, when he compares their sum or product with some figure used as a criterion because known to be correct, although he realizes that compensatory errors may exist.

Such ambiguity is characteristic of, and probably inherent in, index numbers, as is suggested in my *Making of Index Numbers*. To get specific proof as to the size of errors we always need further evidence. Such evidence, in the case of the stock market is easily available. In actual practice the Total Value Criterion does seem to insure a high degree of accuracy for both price and quantity indexes.

the Total Value Criterion, like the Reversal Tests for formula, is a valid and helpful guide. It stands as a solid rock among shifting sands; for a value ratio, given accurate p's and q's (prices and quantities) is not debatable, as is a price index or a quantity index. It is an incontrovertible fact. Though there are always many, and conflicting, indexes purporting to give an average of *price* ratios or *quantity* ratios, and there is always a slight fringe of doubt as to their accuracy, inherent in the very nature of an index[1]; there is only one *value* index and it has no fringe of doubt whatever. Properly speaking, this value index, unlike the price index or the quantity index, is not an index at all, in the sense that price or quantity indexes are; for it is not an average of anything. The value traded in any given period of time is simply the total of all the individual sales values in that period, each sale value being the number of shares of a stock sold in any transaction multiplied by the price in that transaction.

As has been noted, if the Total Value Criterion is *not* satisfied, we may be sure the sampling is inadequate. But the important question, to help settle which the present study is made, is whether, if our samples *do* satisfy the Total Value Criterion, they will also satisfy approximately the other two. If so, we can, in practical work, dispense with all total market calculations except that for total-value. To settle this question it must be put to the test of statistical measurement.

"WANING" AND "WAXING" STOCKS

Before, however, considering samples which do satisfy the Value Criterion, we may first consider those which do not, beginning with the most flagrant cases and then proceeding, by successive stages, toward the satisfying case.

For convenience let us, with respect to each index between two successive months, call the earlier month "0" and the succeeding month "1." Let us first find the one hundred best sellers, or "leaders," in each of these two successive months, that is, the one hundred stocks which, of all the stocks on the New York Stock Exchange, have the highest sales values in the month concerned. We thus have two lists, each comprising one hundred stocks, which may be designated as all-100-leaders of month "0" and all-100-leaders of month "1." Let us now exclude from the all-100-leaders-of-"0" those stocks which are to be found repeated in the list of all-100-leaders-of-"1," thus leaving only those found in the "0" list which, thereafter, wane in importance so as to lose their right to be included among the "1" leaders. Using

[1] See *The Making of Index Numbers*, p. 361.

only this list of "waning" stocks, let us now construct indexes connecting month "0" and month "1."

Such a selection (confined to the waning stocks only) might, perhaps, be suspected *a priori* of being very unfair sampling; but, until the actual calculation has been made, no one would realize how exceedingly unfair it is.

Table II shows the values traded, in dollars, of these "waning" stocks month by month throughout the years 1925 and 1926. We see, for instance, by column (1) that the trading in January amounted to 98 million dollars in those stocks which were included among the 100

TABLE II

VALUE TRADED OF "WANING" STOCKS

INDUSTRIAL COMMON STOCKS

$(000,000)

	Month	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1925	Jan.	98											
	Feb.	42	61										
	Mar.		28	103									
	Apr.			27	60								
	May				19	157							
	June					46	75						
	July						37	116					
	Aug.							36	99				
	Sept.								48	161			
	Oct.									105	201		
	Nov.										87	151	
	Dec.											58	126
1926	Jan.	116											50
	Feb.	58	120										
	Mar.		40	84									
	Apr.			23	53								
	May				18	56							
	June					33	98						
	July						39	84					
	Aug.							44	107				
	Sept.								42	75			
	Oct.									28	117		
	Nov.										28	71	
	Dec.											36	

leaders in January but "waned" or faded from leadership in February; while the trading in these identical stocks in February was only 42 million dollars.

The upper numbers are underscored to denote leadership; the lower ones denote loss of leadership. So likewise we see, by column (2), that 61 million dollars was traded in February by the stocks then leaders as against 28 million dollars in March by the identical stocks after their leadership was lost. These and the succeeding columns show that the values traded in the earlier, or "0," month are usually much more than the values traded in the following, or "1," month.

In like manner, Table III shows the value traded of the stocks that

are "waxing" from month to month. The "waxing" stocks are those which have risen to leadership in month "1" but which were not leaders in the preceding, or "0" month. In this case the *second* month's figure is the one underscored. With the waxing stocks, the trading in the "0" month is usually much *less* than in the "1" month.

Tables II and III give the actual dollar figures of the "waners" and "waxers." Each month-to-month comparison of these figures, reduced to percentages, forms an index-link. These successive links are given in Table IV.

TABLE III

VALUE TRADED OF "WAXING" STOCKS

INDUSTRIAL COMMON STOCKS

$(000,000)

	Month	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1925	Jan.	62											
	Feb.	117	30										
	Mar.		68	40									
	Apr.			65	16								
	May				75	57							
	June					138	34						
	July						129	36					
	Aug.							119	31				
	Sept.								154	43			
	Oct.									203	77		
	Nov.										149	68	
	Dec.											168	53
1926	Jan.	47											151
	Feb.	131	43										
	Mar.		95	39									
	Apr.			76	20								
	May				46	22							
	June					90	39						
	July						142	29					
	Aug.							164	57				
	Sept.								119	33			
	Oct.									76	23		
	Nov.										64	24	
	Dec.											100	

It will be seen at once that the "waning" stocks are uniformly lower than the total market in values, prices and quantities; while the "waxing" stocks are consistently higher. These constant biases suggest at once that both these groups are totally unfit to represent the total market, and further, that uniting the separate month-to-month links into a chain index for the entire twenty-four month period, would give very erroneous indexes for December, 1926, thus chained to the base, January, 1925. Just how grotesquely erroneous these chain indexes are can scarcely be comprehended without actually constructing them. For the purpose of showing the inaccuracy of comparing rapidly and violently changing prices or quantities over relatively long periods of time and especially of comparing them by means of chain

indexes, I have had chain indexes constructed of these "waning" and "waxing" stocks. The results are startling! For December, 1926, on January, 1925, as 100, we obtain as the value index for the total market, 126.6; for the "waners," 0.000,000,05; for the "waxers," 3,776,900,000,000! The quantity indexes are 72.2 for the total market; 0.000,000,06 and 1,086,900,000,000 for the "waners" and "waxers" respectively! Of course these absurd index numbers have no relation to average conditions on the stock market. They are cited here merely to indicate the full horror of this particularly horrible example of bad sampling. The price chain indexes of the "waners" and the "waxers"

TABLE IV

MONTHLY INDEX LINKS OF VALUE, PRICE AND QUANTITY

INDUSTRIAL COMMON STOCKS

Months Linked	Value			Price			Quantity		
	Total Market (1)	"Waning" Leaders (2)	"Waxing" Leaders (3)	Total Market (4)	"Waning" Leaders (5)	"Waxing" Leaders (6)	Total Market (7)	"Waning" Leaders (8)	"Waxing" Leaders (9)
1925									
Jan.-Feb.	84.4	42.9	187.3	103.1	99.4	104.4	81.9	43.1	179.4
Feb.-Mar.	106.2	45.5	224.3	98.3	97.2	102.2	108.1	46.8	219.4
Mar.-Apr.	70.3	285.3	163.2	99.1	93.2	102.9	70.9	28.5	158.6
Apr.-May	117.1	314.8	459.9	103.5	102.8	111.9	107.9	30.6	411.0
May-June	83.5	294.8	243.2	103.6	104.9	112.1	80.6	28.1	216.9
June-July	117.6	48.6	376.5	106.9	99.9	104.9	110.0	48.7	358.9
July-Aug.	102.9	31.2	331.7	103.7	99.8	108.2	99.2	31.2	306.6
Aug.-Sept.	128.4	48.5	492.0	105.2	101.1	107.1	122.0	48.0	459.3
Sept.-Oct.	154.8	65.3	466.8	110.9	102.8	110.3	139.6	63.5	423.3
Oct.-Nov.	116.8	43.3	192.6	104.1	100.9	106.5	112.3	42.9	180.9
Nov.-Dec.	67.6	38.1	275.8	100.4	98.4	110.0	67.4	38.7	251.0
1926									
Dec.-Jan.	68.8	39.4	247.0	103.2	97.0	106.6	66.1	40.6	231.7
Jan.-Feb.	115.6	48.1	282.6	99.4	94.5	102.1	116.3	50.9	276.8
Feb.-Mar.	108.0	331.5	222.2	90.9	89.0	92.6	118.8	37.3	239.9
Mar.-Apr.	65.2	267.6	192.9	94.1	95.1	99.9	69.3	28.1	193.1
Apr.-May	74.7	33.5	238.9	99.6	100.6	105.0	75.0	33.3	227.5
May-June	129.4	60.1	402.6	105.9	101.6	109.6	122.2	59.1	367.4
June-July	144.5	40.1	366.5	109.7	102.2	108.1	131.8	39.3	339.0
July-Aug.	119.5	52.6	562.6	108.3	100.0	113.2	110.4	52.6	497.2
Aug.-Sept.	91.5	39.6	209.4	102.2	99.0	101.8	89.5	40.0	205.8
Sept.-Oct.	111.3	37.6	225.8	98.9	96.9	100.0	112.6	38.8	225.8
Oct.-Nov.	74.0	22.2	275.3	99.2	97.0	106.2	74.6	22.9	259.4
Nov.-Dec.	119.0	50.1	417.4	104.4	101.1	104.7	113.9	49.6	398.6

are by no means so bad as the value and quantity indexes; but they are bad enough, being 175.4 for the total market, 75.9 for the "waners," and 347.5 for the "waxers."

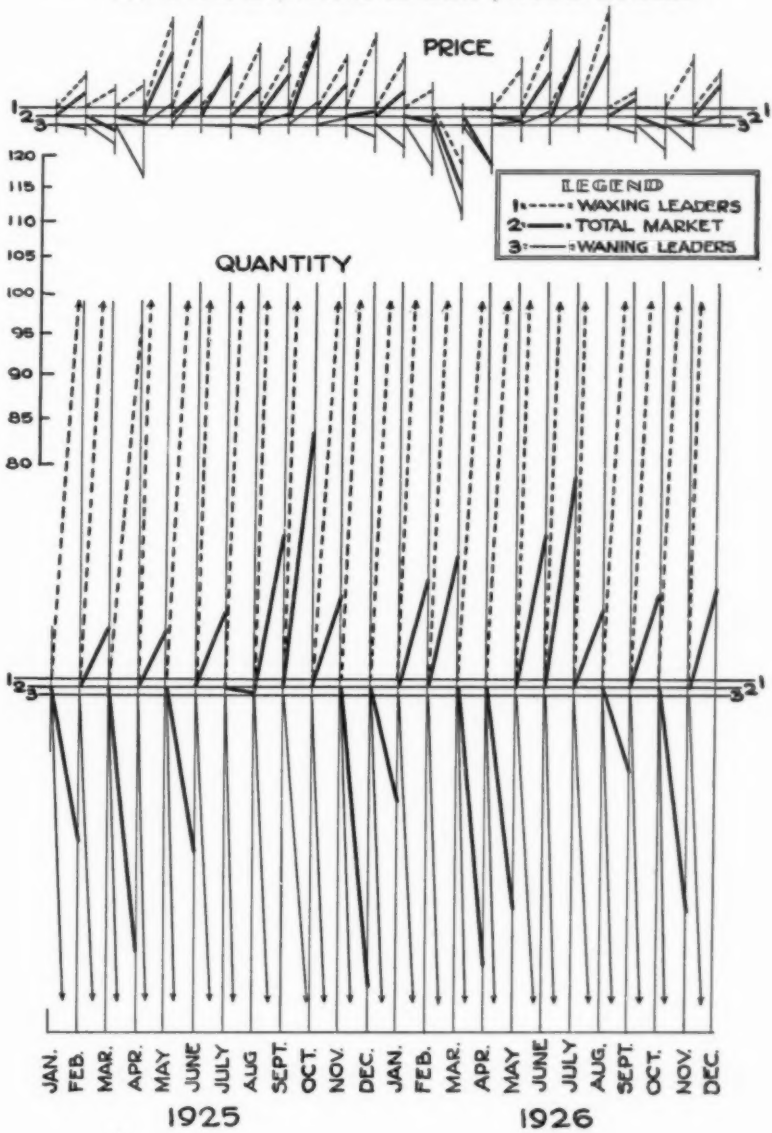
Were we to calculate chain indexes in like fashion week-to-week instead of month-to-month, the divergence between "waners" and "waxers" would cumulate even faster. With day-to-day links, it would pile up still faster. It is worth noting that these "waners" and "waxers" make up on an average each month about 20 to 25 per cent of the 100 "leaders"; but in value traded they each constitute only about 4 per cent of the total market.

Chart II depicts in graphic form the relationships recorded in Table IV, of the prices and quantities of industrial common stocks for the total market, the "waning" and the "waxing" stocks. The value indexes correspond so closely to the quantity indexes that they are not shown on the graphs.

CHART II

MONTHLY INDEX-LINKS OF PRICE AND QUANTITY

TOTAL MARKET, WANING LEADERS, WAXING LEADERS



We find, then, that both the "waning" and the "waxing" stocks as samples of the total market fail utterly to conform to the Total Market Value Criterion (Table IV, column 1); fail utterly to conform to the Total Market Quantity Criterion (column 7); and even fail, only a little less dismally, to conform to the Total Market Price Criterion (column 4).

ALL-100-LEADERS-OF-"0" AND ALL-100-LEADERS-OF-"1"

The second set of sampling, which will now be considered, consists simply of the all-100-leaders-of-"0" or the all-100-leaders-of-"1," without any exclusions. The all-100-leaders-of-"0" evidently include two parts, namely the 20 to 25 highly unrepresentative stocks just described as "waning" and the remaining stocks which are common to, or are repeated in, both lists of leaders. These may be called "continuing" leaders.[1]

The "continuers," unlike the "waners" or the "waxers," have very little bias in either direction. But when we combine these "continuers" with the highly biased "waners" or "waxers" there tends to be a bias imparted to the combined group.

Table V shows the contrast between the indexes made from these "0" and "1" leaders, while Chart III gives the graphs of the price and

TABLE V

MONTHLY INDEX LINKS OF VALUE, PRICE AND QUANTITY

INDUSTRIAL COMMON STOCKS

Months Linked	Value			Price			Quantity		
	Total Market (1)	All 100 "0" Leaders (2)	All 100 "1" Leaders (3)	Total Market (4)	All 100 "0" Leaders (5)	All 100 "1" Leaders (6)	Total Market (7)	All 100 "0" Leaders (8)	All 100 "1" Leaders (9)
1925									
Jan.-Feb.	84.4	80.8	87.0	103.1	103.3	103.6	81.9	78.2	84.0
Feb.-Mar.	106.2	105.1	111.1	98.3	98.3	98.5	108.1	106.9	112.8
Mar.-Apr.	70.3	67.5	72.7	99.1	99.1	99.5	70.9	68.2	73.1
Apr.-May	117.1	105.3	115.2	108.5	108.7	109.0	107.9	96.9	105.7
May-June	83.5	77.5	93.0	103.6	105.3	105.8	80.6	73.6	87.9
June-July	117.6	106.5	119.9	106.9	104.1	104.4	110.0	102.4	114.9
July-Aug.	102.9	98.8	112.5	103.7	104.4	104.9	99.2	94.6	107.3
Aug.-Sept.	128.4	118.7	134.2	105.2	105.1	105.4	122.0	112.9	127.3
Sept.-Oct.	154.8	142.2	160.3	110.9	111.5	112.1	139.6	127.5	143.0
Oct.-Nov.	116.8	116.7	127.7	104.1	104.6	104.9	112.3	113.5	121.8
Nov.-Dec.	67.6	60.6	67.6	100.4	99.8	100.6	67.4	60.7	67.3
1926									
Dec.-Jan.	88.8	82.8	90.3	103.2	103.4	103.9	86.1	80.1	87.0
Jan.-Feb.	115.6	112.8	122.7	99.4	99.4	99.8	116.3	113.5	123.0
Feb.-Mar.	108.0	107.2	144.8	90.9	90.8	91.0	118.8	118.1	158.8
Mar.-Apr.	65.2	64.1	67.6	94.1	93.7	93.9	69.3	68.4	72.0
Apr.-May	74.7	71.6	75.3	99.6	99.6	99.7	75.0	71.9	75.6
May-June	129.4	123.4	133.4	105.9	106.5	106.8	122.2	115.9	124.9
June-July	144.5	143.6	157.5	109.7	110.4	110.6	131.8	130.2	142.4
July-Aug.	119.5	113.5	122.5	108.3	108.4	109.0	110.4	104.7	112.4
Aug.-Sept.	91.5	89.0	93.8	102.2	102.4	102.4	89.5	86.9	91.6
Sept.-Oct.	111.3	110.9	115.2	98.9	99.1	99.2	112.6	111.9	116.1
Oct.-Nov.	74.0	71.1	75.5	99.2	99.0	99.2	74.6	71.9	76.1
Nov.-Dec.	119.0	113.5	120.4	104.4	104.6	104.9	113.9	108.3	114.6

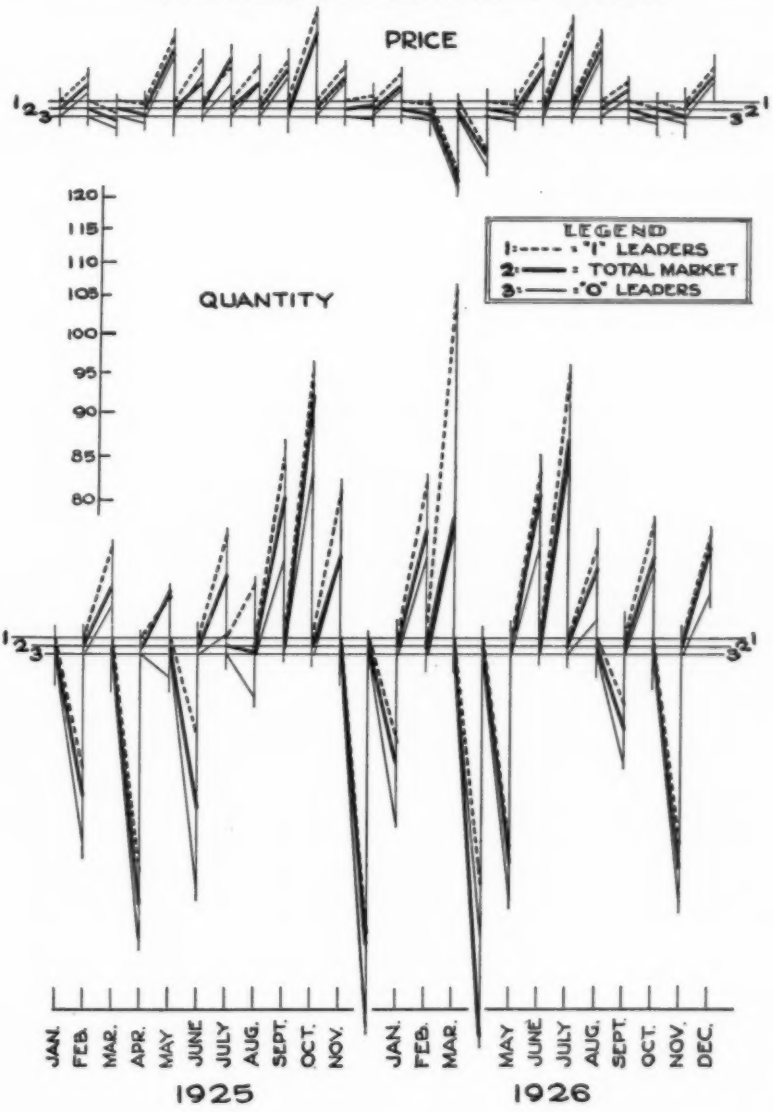
[1] That is, all-100-leaders-of-"0" include the "waners" and the "continuers," and all-100-leaders-of-"1" include the "waxers" and the "continuers."

quantity indexes. For these the contrasts are not great and the index of all-100-leaders-of-"0" is excellent. In fact, it agrees with the Total Market Criterion for prices slightly better than the other indexes I have found, except one.

CHART III

MONTHLY INDEX-LINKS OF PRICE AND QUANTITY

TOTAL MARKET, "0" LEADERS AND "1" LEADERS



We find, then, in the stock market at least, a distinct—and often colossal—bias, up or down, according to the character of the sampling.

This *sampling* bias is a new concept in Index Numbers and is quite distinct from the *formula* bias (whether "type" bias or "weight" bias) discussed in *The Making of Index Numbers*. To indicate a reliable method of insuring, to a reasonable extent, against sampling bias is the chief objective toward which this article is aimed.

CONTINUING LEADERS

The next sampling to be studied is based on the "continuers" in the two lists of leaders (for each successive pair of months).

Table VI and Chart IV show that the indexes based on the "continuers" are good indexes.

TABLE VI

MONTHLY INDEX LINKS OF VALUE, PRICE AND QUANTITY

INDUSTRIAL COMMON STOCKS

Months Linked	Value		Price		Quantity	
	Total Market (1)	"Continuing Leaders (2)	Total Market (3)	"Continuing Leaders (4)	Total Market (5)	"Continuing Leaders (6)
1925						
Jan.-Feb.	84.4	83.1	103.1	103.5	81.9	80.3
Feb.-Mar.	106.2	108.2	98.3	98.4	108.1	110.0
Mar.-Apr.	70.3	70.4	99.1	99.3	70.9	70.8
Apr.-May	117.1	109.7	108.5	108.9	107.9	100.8
May-June	83.5	84.8	103.6	105.2	80.6	80.6
June-July	117.6	111.0	106.9	104.3	110.0	106.4
July-Aug.	102.9	105.6	103.7	104.7	99.2	100.9
Aug.-Sept.	128.4	124.6	105.2	105.6	122.0	118.0
Sept.-Oct.	154.8	150.9	110.9	112.2	139.6	134.5
Oct.-Nov.	116.8	125.5	104.1	104.8	112.3	119.7
Nov.-Dec.	67.6	62.0	100.4	99.9	67.4	62.0
1926						
Dec.-Jan.	88.6	85.8	103.2	103.7	86.1	82.7
Jan.-Feb.	115.6	117.8	99.4	99.7	116.3	118.2
Feb.-Mar.	108.0	112.8	90.9	90.9	118.8	124.1
Mar.-Apr.	65.2	65.4	94.1	93.7	69.3	69.8
Apr.-May	74.7	73.0	99.6	99.6	75.0	73.3
May-June	129.4	127.1	105.9	106.7	122.2	119.1
June-July	144.5	151.4	109.7	110.7	131.8	136.8
July-Aug.	119.5	116.1	108.3	108.7	110.4	106.8
Aug.-Sept.	91.5	91.1	102.2	102.5	89.5	88.9
Sept.-Oct.	111.3	113.4	98.9	99.2	112.6	114.4
Oct.-Nov.	74.0	73.5	99.2	99.0	74.6	74.3
Nov.-Dec.	119.0	116.2	104.4	104.9	113.9	110.8

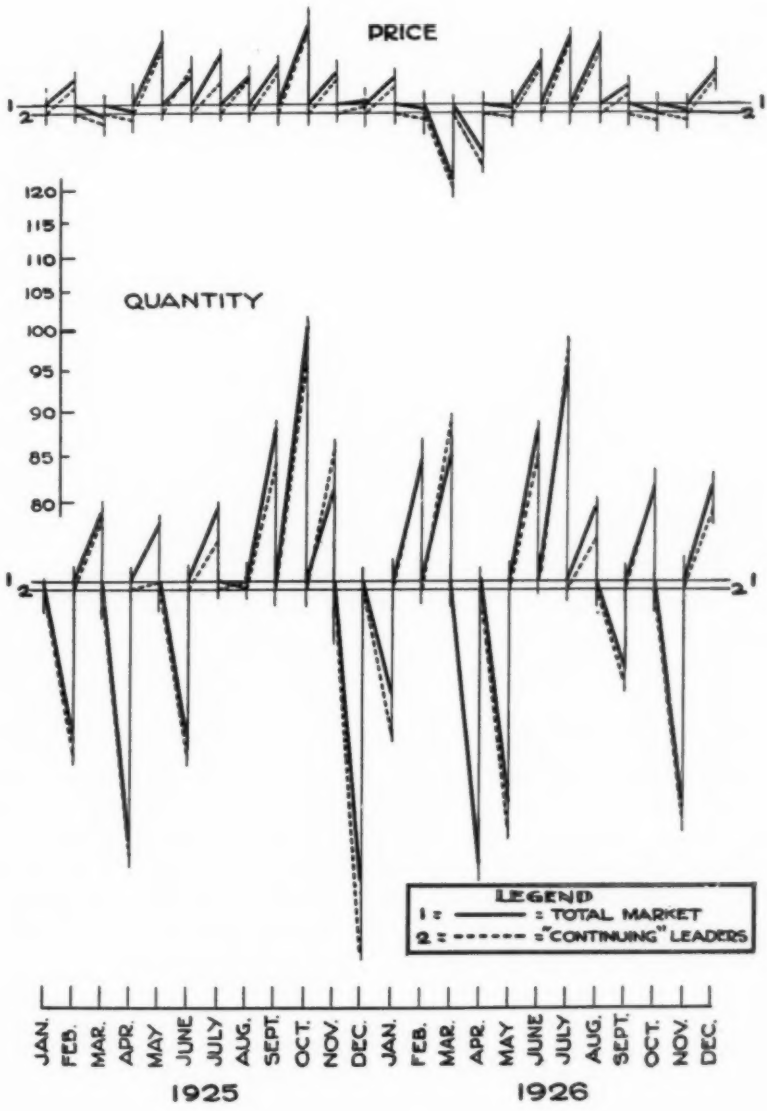
CLOSING THE SCISSORS

Before proceeding further let us survey our progress thus far. We began with the sampling of "waners" and "waxers," the most biased

CHART IV

MONTHLY INDEX – LINKS OF PRICE AND QUANTITY

INDUSTRIAL COMMON STOCKS



sampling of all, and found that our indexes, for both price and quantity, calculated from the "waners" shot rapidly downward; while those calculated from the "waxers" shot rapidly upward.

We next found that if to the "waners" we added the "continuers," thus reaching the original all-100-"0"-leaders the downward bias was overcome. Likewise the addition of these same "continuers" to the "waxers," thereby constituting the whole "1" list, tempers the upward bias.

Finally we found that the "continuers" alone bring all our curves (except the price curve) closer toward the truth, as disclosed by our Total Market Criteria.

That is to say, we began with a pair of curves possessing great upward and downward bias. Even the price curves gaped wide apart while the quantity curves gaped still wider, like wide-open scissors.

Then, by including the "continuers," we closed the scissors part way.

Finally, by employing "continuers" only, we closed the scissors entirely and arrived, approximately, at our three Total Market Criteria.

This closing applies to the price scissors, the quantity scissors and the value scissors. And not only did we, at each successive stage, move our pair of scissors together but (with one exception) we approached closer to the Total Market Criteria. In general, all three indexes move together—either all downward or all upward, though at different rates.

Thus far we have only *approximate* coincidence for all three indexes. But, evidently, by slightly changing the last sampling (consisting of the "continuers") we can easily make at least our value index agree *completely* with the total value criterion. Will such a complete value coincidence bring about a nearly complete price and quantity coincidence? That is our chief question.

COMPLETING THE CONFORMITY TO THE TOTAL VALUE CRITERION

Our next step, then, is to add to the list of "continuing" stocks such stocks, taken either from the "waners" or from the "waxers," as will bring about *complete* conformity to the Total Value Criterion. The addition of stocks from the former will bend all three indexes downward; the addition of those from the latter will bend them all upward.[1]

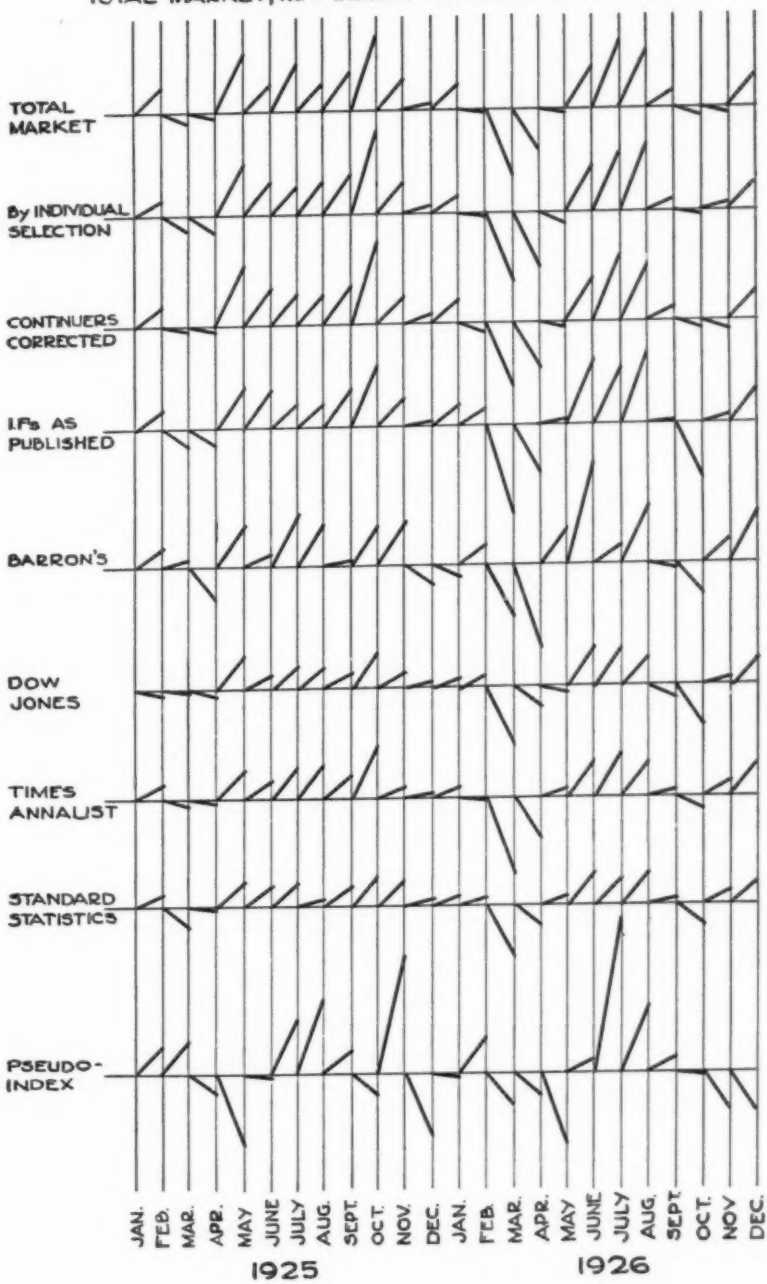
But this use of the "continuers," as a group, amended by a fraction of the "waxers" and "waners" is really only a short cut.

[1] For ease of calculation the additions of stock are made *en masse*, *i.e.*, as a fraction of the entire group, of "waners" or "waxers" as the case may be. It will be noted that, if we were to add the stocks one by one we would ordinarily need, so to speak, to split the last stock to secure complete conformity to the value criterion; for it would seldom happen that an integral number of stocks would exactly satisfy the criterion.

CHART V

MONTHLY INDEX-LINKS OF PRICE

TOTAL MARKET, NEW INDEXES AND PUBLISHED INDEXES



A still closer approximation to our Total Market Criteria may be obtained by first arranging the "continuers" individually in the order of their value ratios for any two successive months. One of the results of this method is given in Table VII and Chart V.

The object of Chart V is to contrast the new price indexes here obtained through the use of the Total Value Criterion with the chief indexes previously published. It will repay careful scrutiny. The month-to-month index links of each index number should be compared vertically with the corresponding index links of the Total Market Price Index. The indexes are arranged in the order of their closeness of conformity to the total market. Next below the Total Market Price Index comes the monthly index links constructed from samples selected from among the "continuers" on the basis just mentioned. For each month-to-month index all the stocks are first arranged in the order of their value ratios and any group of contiguous stocks in this list is taken which, as a group, conforms to the Total Value Criterion (including for that purpose any necessary fraction of a stock at either end of the group). All such groups give excellent results; in general, the larger the group the better. The one shown in the chart is the best. It comprises next to the largest group thus selected (about 65 stocks). As shown by the standard deviations method its links are slightly nearer those of the criterion than those of any other curve investigated.

Next below this best approximation is the index obtained by the short cut. It includes all the "continuers" together with a fraction either of the "waxers" or the "waners" as needed to produce conformity to the Total Value Criterion. Next comes my own index as actually published and before the Total Market Criterion was developed.[1]

Next in order comes Professor Crum's index published in *Barron's Weekly*. Then follow Dow-Jones, the Times Annalist and Standard Statistics indexes. Finally, at the bottom, is the average price of all industrial stocks previously mentioned.

There are, of course, many other indexes which might be included. It is to be noted that all the curves, only two of which are included in the chart, based on samples which are taken principally from the "continuers" and which satisfy the Total Value Criterion, give better results, as judged by the Price Criterion, than those of samples which do not satisfy the Total Value Criterion.

Attention is called especially to the almost perfect conformity to the Price Criterion of the *individual links* of those curves which satisfy

[1] The method of its construction varied slightly from time to time. In the beginning it included the entire list of leaders in both months "0" and "1"; later it employed the "continuers" only.

the Total Value Criterion as contrasted to the irregularities of the other curves even when for the whole span they have a good trend.

It will be noted that the widely used indexes of Dow-Jones and the Times Annalist deviate widely from the Total Market Criterion at the top. These and other currently used indexes have a tendency to sag as compared with the index for the total market. Presumably this sagging is due to the almost inevitable obsolescence or "waning" of stocks in any fixed list even when its fixity is somewhat fictitious because it is continually revised by substitutions.

Chart VI compares similarly various indexes, new and old, of *quantities*, or activity, with the Total Market Quantity Criterion. Here we have only one well-established and widely used quantity index, the number of shares sold. This is next to the bottom curve. The

TABLE VII

MONTHLY INDEX LINKS, INDUSTRIAL COMMON STOCKS

MOST REPRESENTATIVE OF TOTAL MARKET CRITERION

Months Linked	Price			Quantity		
	Total Market (1)	Most Representative Sample* (2)	"Continuers" Adjusted** (3)	Total Market (4)	Most Representative Sample* (5)	"Continuers" Adjusted** (6)
1925						
Jan.-Feb.	103.1	102.0	103.6	81.9	82.7	81.4
Feb.-Mar.	98.3	98.0	98.3	108.1	108.4	108.0
Mar.-Apr.	99.1	97.7	99.3	70.9	72.0	70.8
Apr.-May	108.5	107.6	109.2	107.9	108.8	107.2
May-June	103.6	104.9	105.2	80.6	79.6	79.4
June-July	106.9	104.1	104.6	110.0	112.9	112.4
July-Aug.	103.7	104.6	104.5	99.2	98.4	98.4
Aug.-Sept.	105.2	105.6	105.7	122.0	121.6	121.4
Sept.-Oct.	110.9	112.4	112.6	139.6	137.6	137.5
Oct.-Nov.	104.1	104.2	104.7	112.3	112.1	112.0
Nov.-Dec.	100.4	101.0	100.2	67.4	67.0	67.6
1926						
Dec.-Jan.	103.2	102.2	104.0	86.1	86.9	85.4
Jan.-Feb.	99.4	99.7	99.6	116.3	116.0	116.2
Feb.-Mar.	90.9	91.0	90.8	118.6	118.6	118.9
Mar.-Apr.	94.1	92.3	93.7	69.3	70.6	69.6
Apr.-May	99.6	98.6	99.5	75.0	75.7	75.0
May-June	105.9	106.7	106.6	122.2	121.3	121.2
June-July	109.7	108.4	110.4	131.8	133.4	131.0
July-Aug.	108.3	109.5	109.1	110.4	109.1	109.6
Aug.-Sept.	102.2	101.8	102.5	89.5	89.9	89.3
Sept.-Oct.	98.9	99.5	99.1	112.6	111.9	112.3
Oct.-Nov.	99.2	100.7	99.0	74.6	73.5	74.7
Nov.-Dec.	104.4	104.0	104.5	113.9	114.3	113.9

* Columns 2 and 5 show indexes of a sample built up from the group of "continuing" leaders by starting with that stock whose value ratio agrees most closely with the Total Market Value Index and adding successively stocks whose value ratios are just above and just below the Total Market Value Index so as to keep the value index of the resulting sample in agreement with the Total Market Value Index. Several samples were made in the manner described. The one whose indexes are shown in this table is next to the largest sample made and contains about 65 stocks.

** Columns 3 and 6 show "adjusted" indexes of the "continuing" stocks. The adjustment is made by adding that fraction of the "waning" stocks or the "waxing" stocks necessary to make the value index of the "continuing" stocks conform to the value index of the Total Market.

CHART VI

MONTHLY INDEX-LINKS OF QUANTITIES

TOTAL MARKET, NEW INDEXES AND PUBLISHED INDEXES

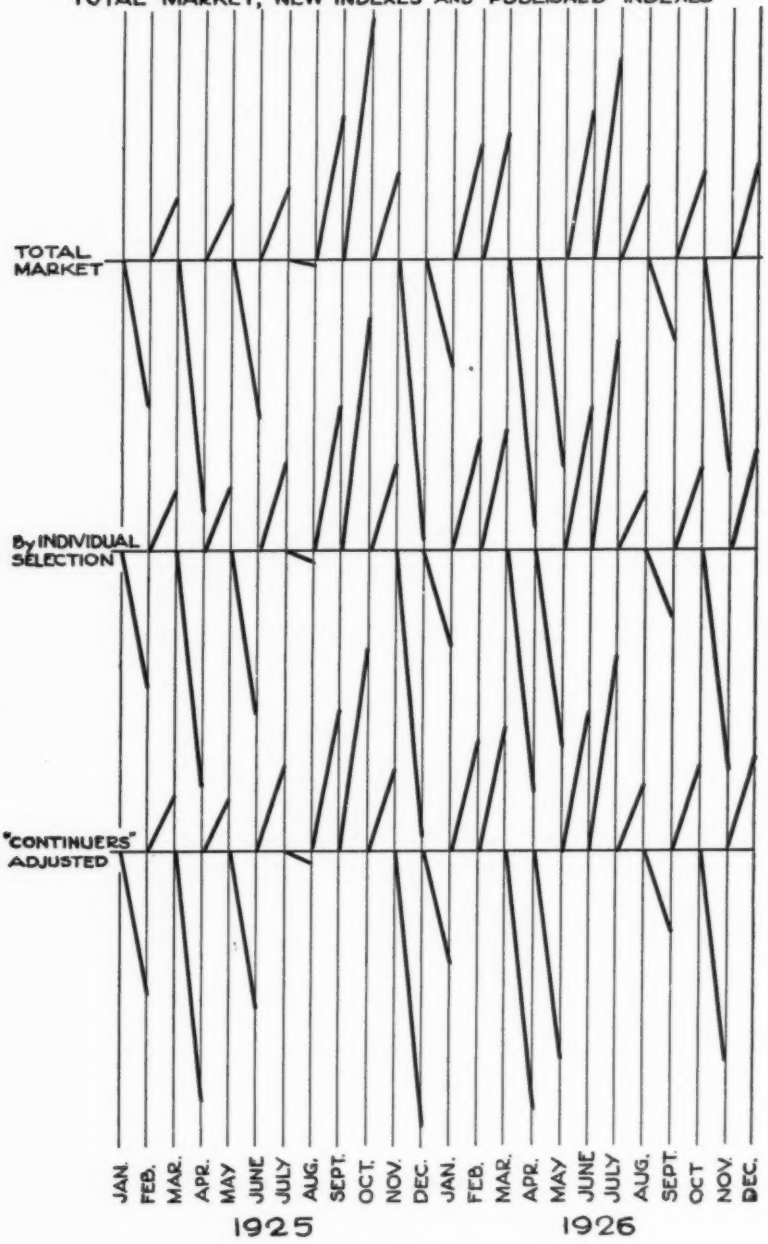
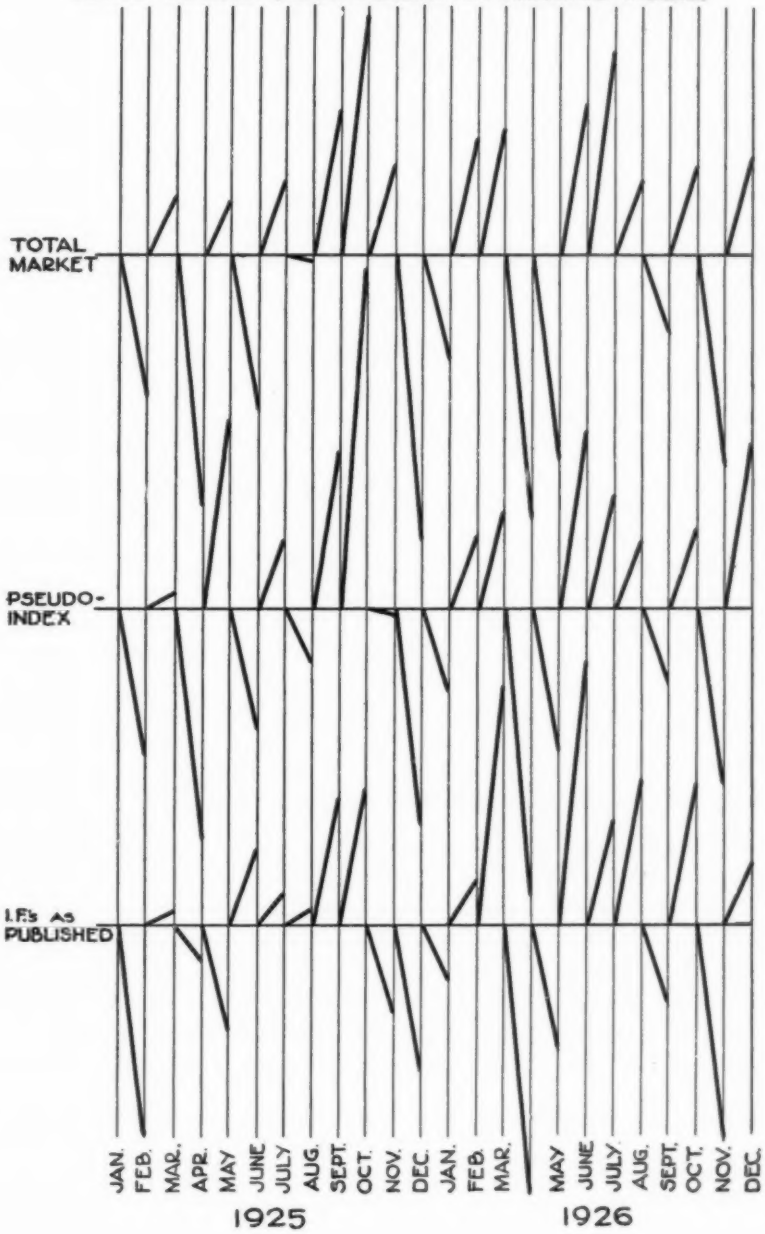


CHART VI (CONT.)

MONTHLY INDEX-LINKS OF QUANTITIES

TOTAL MARKET, NEW INDEXES AND PUBLISHED INDEXES



bottom curve is my own Index of quantities as first published. This turns out, now that I have applied the Total Market Criterion, to be hardly as good an index as the ordinary number of shares sold. Both are inferior to the newer indexes at the top which conform to the Total Value Criterion.

The Total Value Criterion is, of course, not confined, in its application, to the stock market, but is applicable to all cases where indexes of price and quantity are both available. It may be applied to commodity indexes. It is hoped to apply it presently to obtain paired indexes of the prices and production of automobiles. Hitherto, in commodity indexes, the problem of selecting samples has been largely left to arbitrary judgment. Probably no great harm, if any, is thereby done to the price indexes.

But quantity indexes of commodities will probably, unless controlled, be found sensitive to waning or obsolescence. Quantity indexes are still in their infancy. But it may not be long before these will give very untrustworthy comparisons even for short periods of time, if, as seems likely, the same sort of sampling bias applies as we have found in the case of the stock market. The most effective remedy would seem to be the Total Value Criterion.[1]

CONCLUSIONS

We may now summarize the main conclusions of this study.

(1) Every sampling tested which fails to conform to the Total Value Criterion yields a poor index of prices, with one exception (all-100-leaders-of-"0"), and yields a poor index of quantities, with no exception.

(2) Every sampling tested which does conform to the Total Value Criterion yields a good index of prices and a good index of quantities—assuming that the "ideal" formula (or other good formula) is employed.[2]

(3) Most of the current price and quantity indexes are from samples which fail to conform to the Total Value Criterion and, partly for that reason, partly for lack of a good formula, deviate widely from the Total Market indexes for prices and quantities.

These conclusions are all based on studies of the stock market. But this article aims to enunciate a general principle applicable to any sort

[1] When applying this principle to commodities we shall not usually, of course, even have available the total sales but shall have to use, instead, some such indication of the totals as total clearings. Generally we have far better data for values than for quantities.

[2] When we use pseudo-indexes such as the average price of all stocks on a varying list and (for the quantity index) the mere number of shares sold, the results are unsatisfactory even with the best sampling. These two pseudo-indexes, multiplied together, do give the true value index, that is, conform to the Value Criterion; but both are erratic in opposite directions.

of index. In order to determine more exactly the usefulness of the Total Value Criterion, additional statistical analysis is needed. Considerable work on this line has already been done, and more is in progress.

FORECASTING THE VOLUME AND VALUE OF THE COTTON CROP[1]

By Bradford Bixby Smith

The present cotton situation, characterized by a record crop and depressed prices, makes an analysis of the factors influencing cotton prices especially timely. It raises such questions as: What effect has the size of the crop upon prices, upon the value of the crop? What is likely to be the price trend? What acreage changes may be expected? What effect would a change in business conditions have upon prices?

For the purpose of answering such questions the statistician is fortunate in having a large body of basic data over a long period of time such as records of acreage, yield per acre, production, weather conditions, exports, consumption and central market prices. In using such records Professor Moore has done pioneer work. Some ten years ago his *Forecasting the Yield and Price of Cotton* described his correlation of yield per acre with weather conditions during the growing season and his correlation of price with production and price level. In his price correlation, he secured a multiple correlation of 0.859 thus successfully demonstrating that supply and price level (or items correlated with these) are probably the predominant factors in determining the price.

If his experiment is repeated today using logarithms of price, price level and production plus carryover at the beginning of the season, a correlation of price with the other two factors gives a coefficient of 0.955.

Although this does not afford a means of forecasting the price it does show that the price probably cannot be accurately forecasted without first forecasting the size of the crop. For that reason it will be necessary to go into methods of long range forecasting of the size of the crop. But for the present the assumption is made that the size of the crop for any given year can be forecasted sometime about March, for certain valuable relationships can be determined even though it were impossible to forecast the size of the crop.

To make the statistical problem specific, let us suppose that sometime during March, or thereabouts, it is desired to forecast the price of cotton as it will average during the next December and January. The particular price to be forecasted will be the average price during

[1] Presented at the eighty-eighth annual meeting of the American Statistical Association, St. Louis, December, 1926.

December and January of the New York March contract—March futures during December and January. For this particular year, for example, the task would have been to forecast the average of March futures during this month of December and next month, and to have made that forecast sometime last March, assuming for the present that it were known that this year's crop would have been in the neighborhood of eighteen million bales.

To avoid confusion I will refer to this price of cotton being forecasted as the price in December omitting the constant repetition of "average during December and January" which would otherwise be necessary. When the particular year for which the price is being forecasted is referred to I will designate the year by the calendar year which includes the December. Thus the task, in this abbreviated nomenclature, this year would have been to have forecasted the December, 1926, price.

To have made the forecast last March the forecast would obviously have had to be made on the basis of information available at that time. The statistical technique, then, is to take information available prior to March and to ascertain if in past years some systematic relationship between this information and the price in the following December can be established. If in past years we find that certain factors as measured in March have borne systematic relationships to prices in the next December, with great regularity, is it not reasonable to suppose that by applying those relationships to the same factors in a current year it would be possible to forecast the price? Such, at any rate, was the approach to the problem made in this study. With this approach the problem therefore becomes primarily one of ascertaining if relationships sufficiently accurate for forecasting purposes do actually exist between factors as they may be measured not later than March and the subsequent December price.

This process of finding relationships logically consists of two stages: (1) The selection of the information available in March to be used and the type of statistical expression to be given that information; and (2) the determination of whatever statistical relationships may be found between this information and the subsequent price. This second step, thanks to Dr. Ezekiel's development of multiple curvilinear correlation methods, is, to a large degree, mechanical.[1]

In the first place it was thought that the price of cotton in December might very easily bear some relation to the price of cotton in the preceding December, for, if costs of production were gradually increasing,

[1] "A Method of Handling Curvilinear Correlation for Any Number of Variables," by Mordecai Ezekiel, this JOURNAL, Vol. XIX, No. 148, December, 1924.

a relatively higher price might be expected to be maintained, or, if demand had undergone an increase—perhaps in response to a rising business cycle—that increase, or some portion of it, might be expected to persist throughout the year and to have some vestigial influence upon the price a year later. So the first series used in forecasting the December price was the same price taken one year earlier.

The second series selected for use in forecasting the December price was the Bureau of Labor Statistics index of All Commodity prices at wholesale during the preceding December (and January). This was selected because it is logical to suppose that the price of cotton is related to the price of other commodities. The most appropriate index number to use would be the index for the December of the price we are trying to forecast rather than for the December a year earlier. But since this forecast is to be made considerably in advance of that time, that index number is not known. The index a year earlier is, therefore, only an approximation; but it is available.

The third series selected for use in forecasting the December price was the available supply of cotton in the United States at the end of the preceding February. A large supply would, of course, tend to keep the price down, a small supply would probably be used up before the year was out and lay the ground for an increased price. The particular supply figure used was defined as the sum of the preceding year's crop and the carryover at the beginning of the season less the sum of exports and consumption for the season up to the end of February. It is essentially the net available amount of ginned cotton in this country which may be consumed or exported, in millions of bales.

If this supply were used up more rapidly than usual an increased price would be expected and vice versa. Hence it is desirable to use a series which indicates the level of industrial activity for the next twelve months beginning with the first of March. A suitable series would be one which forecasts, let us say, the average percentage of blast furnaces in blast for this next twelve months period. Such a series has been constructed which forecasts with an accuracy measured by a correlation of 0.94. I will not take the time to explain it at this point, but will describe it later when I take up methods of long range forecasting of the size of the crop—it is long range forecasting of industrial activity. Suffice that in forecasting the price of cotton in December a series was used which forecasts the level of industrial activity for the twelve months beginning with the preceding March. This forecast series is available in March.

As was stated earlier, for the purposes of this study it was assumed that the crop can be forecasted sometime in March. The fifth series

used in forecasting the price of cotton during December was, accordingly, the size of the current crop—in millions of bales.

Before correlating these various series to ascertain if systematic relationships existed, all the series were expressed in logarithms. This was done because it was felt that if relationships existed among these items, the relationships would be more easily ascertained between proportional changes in the items rather than between absolute changes in the items.

A final series introduced in the analysis was one to designate the time relationship of one observation to the others, in order to determine any net trend movement existing in the logarithm of the price. This series was nothing but the last two digits of the calendar year.

Summarizing, the logarithm of the price of cotton during December is the item to be forecasted, and the items by means of which the forecast is to be made are:

(1) The last two digits of the calendar year in which the December falls.

(2) The logarithm of the size of the crop when expressed in millions of bales.

(3) The logarithm of the available supply of cotton in the United States at the end of the preceding February, expressed in millions of bales.

(4) The logarithm of the price in December (and January) a year preceding the given December.

(5) The logarithm of the Bureau of Labor Statistics index in December (and January) a year preceding the given December.

(6) The logarithm of the forecast of the average percentage of furnaces in blast for the year beginning the March preceding the given December.

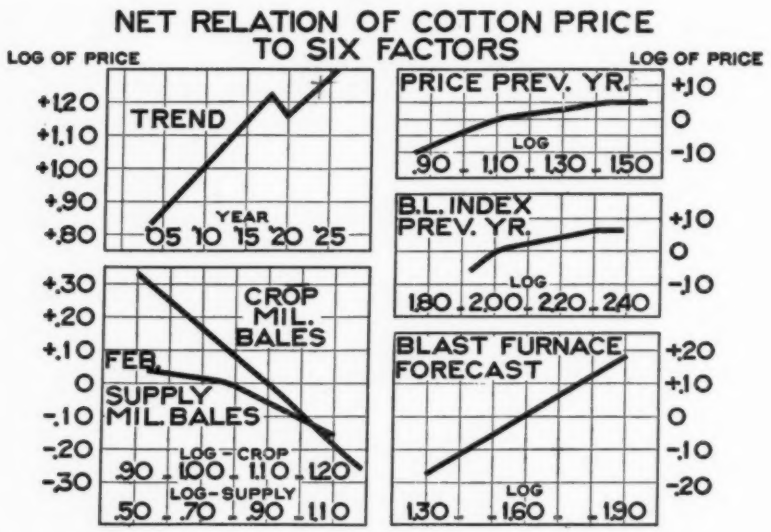
The method of determining the relationships involved was to treat the December price as the dependent variable and the other series as independent variables in a multiple curvilinear correlation analysis covering the period 1903 to date. The multiple correlation index proved to be 0.965 and the net regressions of the dependent upon the various independent variables are shown graphically in Chart I.

The first of these curves shows the net trend movement in the logarithm of the price. Its interpretation is that, the other items measured being equal, the price of cotton would increase about five and a half per cent a year, owing to factors measured only by reason of their correlation with the passage of time. Putting it another way it might be said that demand has increased at a rate sufficient to justify an increasing

cost of production of about five and a half per cent a year. Since the price of cotton at an earlier date is one of the independents, the interpretation might also be that the amount of change taking place during the year has been increasingly greater.

The second curve shows the relationship between the logarithm of the price and the logarithm of the size of the crop. In this case an increase in the logarithm of the size of the crop of 0.30 results in a decrease in the logarithm of price of 0.52; or in terms of original items, doubling the

CHART I



crop results in a third of the price. The price changes in an inverse direction approximately 1.6 times as fast as the crop.

The next of the curves has to do with the relation of the logarithm of the price to the logarithm of the supply at the end of the preceding February. Since the effect of this supply would tend to be evaluated during the intervening period we would expect it to have less influence upon the price than does the crop. And so it proves to be—the slope of the line is much less despite the reduced scale showing that it takes a much greater proportional change in supply to have a given effect upon price than is required in the crop.

The next curve shows the relation between the logarithm of the price of cotton in December and the same price a year earlier. This curve is about as would be expected. If prices are high this year there is something of a tendency for prices to remain high next year. If we had a two year business cycle rather than a three or four this relationship

might be reversed, or on the other hand, if changes in costs of production were transitory and peculiar to one season only, a reversed influence might be expected.

The curve showing the relationship between the logarithm of the price and the logarithm of the Bureau of Labor Statistics index a year earlier has a contour similar to that of the price of cotton relation curve just described, and, I think, may be explained much along the same lines: A period of high price level tends to last more than a year.

The last curve shows the relationship between the logarithm of the forecasted percentage of furnaces in blast and the logarithm of the price and proves to be precisely as would be expected: A high level of industrial activity means a high price and vice versa.

These, then, represent the net relationships between the price of cotton in December and these other factors over the last twenty-three years. To see if these relationships would give satisfactory forecasts we might try them out in previous years. Thus let us suppose that in March, 1924, it was desired to forecast the average price in the next December. From the net trend curve for 1924 the logarithm of price indicated is 1.25. The crop that year was 13.63 million bales, the logarithm is 1.135 and reading from the curve the effect on price was $-.06$. By similarly reading the effects of the other variables for that year and summing these effects a forecast of the logarithm of price proves to be 1.37. Taking the antilogarithm gives a forecasted price of 23.4 cents. The actual price turned out to be 23.9 cents. If this process is repeated for every year in the period covered and the estimates so secured are charted on a graph containing also the actual price as it proved to be, the picture would look like Chart II. During the violently disturbed years of 1918 to 1920 the forecasts were not very good, although they would not have been seriously misleading as to the direction of price change to be expected. For the rest of the period there is a remarkably close agreement between the actual price in December and the forecasts of that price made some ten months earlier. The correlation between the two for the entire period is 0.95; if the 1919 year is omitted when the December came in the midst of a speculative boom the correlation rises to 0.97.

All this, however, it must be remembered has been on the assumption that it was possible to forecast the size of the crop. Nevertheless various sizes of crops may be substituted in the process of making forecasts currently and a series of estimated prices be secured corresponding to the different sizes of crop substituted, and some rather useful information be obtained. Thus, this information would be useful in the present cotton situation in order to find out how great the crop re-

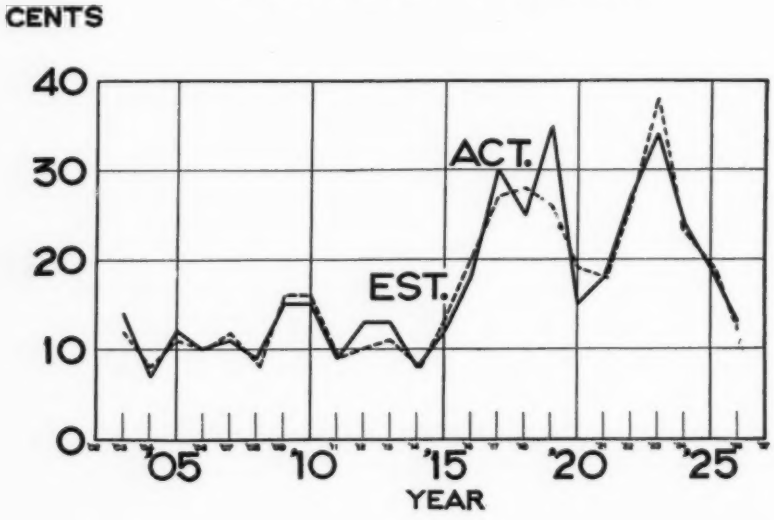
duction must be in order to bring about a given price. It furnishes a method of evaluating influences other than the crop rather accurately and thus confines the estimating hazard rather completely to the estimating of the size of the crop.

The size of the crop may be forecasted with a surprising degree of accuracy almost before the crop is planted. This is essentially a double process: The forecasting of the acreage and the forecasting of the average yield per acre.

Taking up first the forecasting of the acreage this may be accom-

CHART II

COTTON PRICES



plished on the basis of the relationship between acreage changes and prices during the preceding winter. The relationship is very close.

The particular item which was selected for forecasting was the percentage increase or decrease in the acreage harvested in one year compared with the acreage harvested in the preceding year. This percentage increase or decrease was treated as the dependent variable in a correlation analysis. The independent variables were as follows.

In the first place it was thought that high cotton prices relative to the prices of other farm products during the period when the producers are deciding on their crop program for the next year would tend to induce an expansion in acreage and low prices would induce a contraction.

Hence the December price of cotton divided by the Bureau of Labor Statistics index of Farm Product prices at wholesale was taken as the first series. The particular cotton price used was the average price during December of cotton for delivery the next March in New York.

The second series used was exactly the same as the one just described, except that it was taken for one year earlier, in order to care for any "hold-over" effect of price from one year to the next.

The third series by means of which the change in acreage taking place in the coming season was explained was the actual change in acreage that took place in the season just past. For, other things being equal, some reaction might be expected.

Finally, owing to changing costs of production, changing values of farm products with reference to other products, and other influences, it is easily probable that the price necessary to stimulate an acreage increase may have been gradually rising or falling through the period included in the study. Hence it is desirable to include as a final factor in the analysis of influences determining acreage changes a series which represents a given year in its time relation to other years. This series is conveniently constructed by merely taking the last two digits of the calendar years designating the year in which the forecasted acreage was harvested.

Summarizing, the dependent variable is the percentage increase or decrease in the United States acreage harvested in any given calendar year compared with the preceding year. The independent variables are:

(1) The last two digits of the calendar year designating the year of harvest.

(2) The price of cotton in the preceding December divided by the corresponding Bureau of Labor Statistics index of Farm Products.

(3) The price of cotton in December in the calendar year two years preceding the given year divided by the corresponding Bureau of Labor Statistics index of Farm Products.

(4) The percentage increase or decrease that took place in the acreage harvested the preceding year.

These items were correlated for the period 1902 to 1926, which resulted in a multiple correlation index of 0.95. If the one year, 1910, is omitted, the correlation is 0.98.

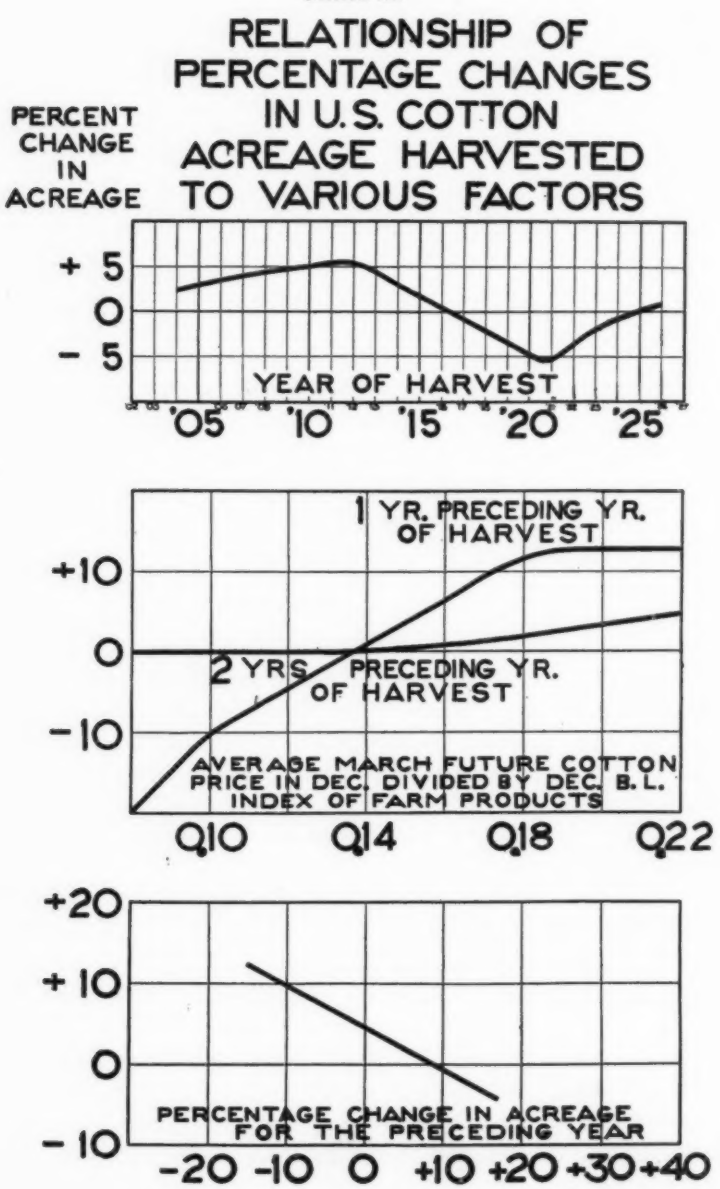
The net relations of the dependent to the four factors are shown in Chart III.

The first of these curves shows the relation of the percentage acreage change to the net trend factor. From 1902 to 1912 this curve indicates a growing increase in acreage to be normal. During the spread of the boll weevil and the World War period there was a change in the normal

trend change in acreage from an increase of about 5 per cent in 1912 to a decrease of about 6 per cent in 1921. After 1921 the pre-war trend was resumed.

The curves showing the relationship of the relative price one year

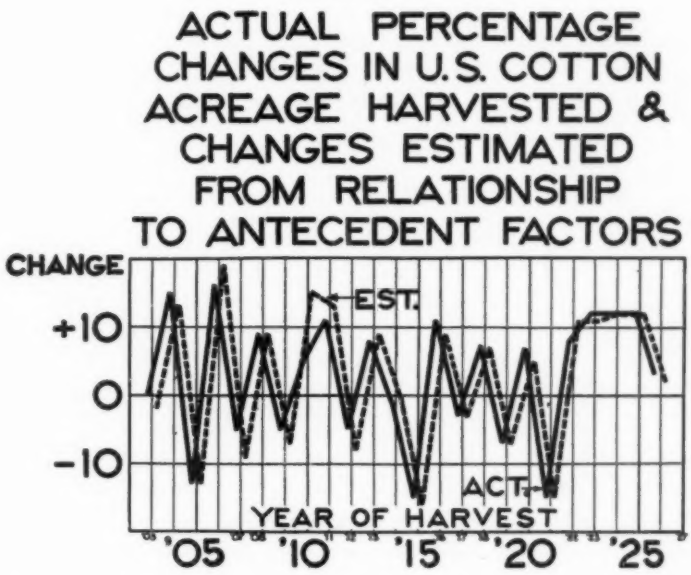
CHART III



before and two years before to acreage changes are about as would be expected. The curve representing the immediately preceding December price is steeper than the other, thus indicating that a given change in this price has much greater effect upon subsequent acreage than the same change in the other.

The last curve shows the relation between acreage change in the preceding year and acreage change in the given year. It has a slope as

CHART IV



would be expected—the greater the increase in the past year, the greater the reaction or decrease in the given year.

To employ these relationship curves for the purpose of forecasting, it is only necessary to obtain the values of the independents for the given year, read from the curves their effects upon acreage, and add together the resulting readings. The sum will represent the estimated, or forecasted, acreage change. This forecast may be secured at the beginning of the year to forecast the acreage change that will take place in the next season, as compared with the acreage just picked.

To test the accuracy of these relationships for forecasting purposes they may be tried out in previous years for which we know the actual acreage changes. If forecasts are constructed on the basis of these relationships for the past twenty-five years and compared with the actual acreage changes, the picture would look like Chart IV. The agreement is astonishingly close.

So much for the matter of forecasting acreage changes approximately a year in advance. Now to take up the matter of forecasting the average United States yield per acre of lint cotton before the crop is even planted. The combination of such a forecast with the acreage forecast just described obviously furnishes a method of forecasting the size of the crop which may be used in forecasting the price which I have previously described.

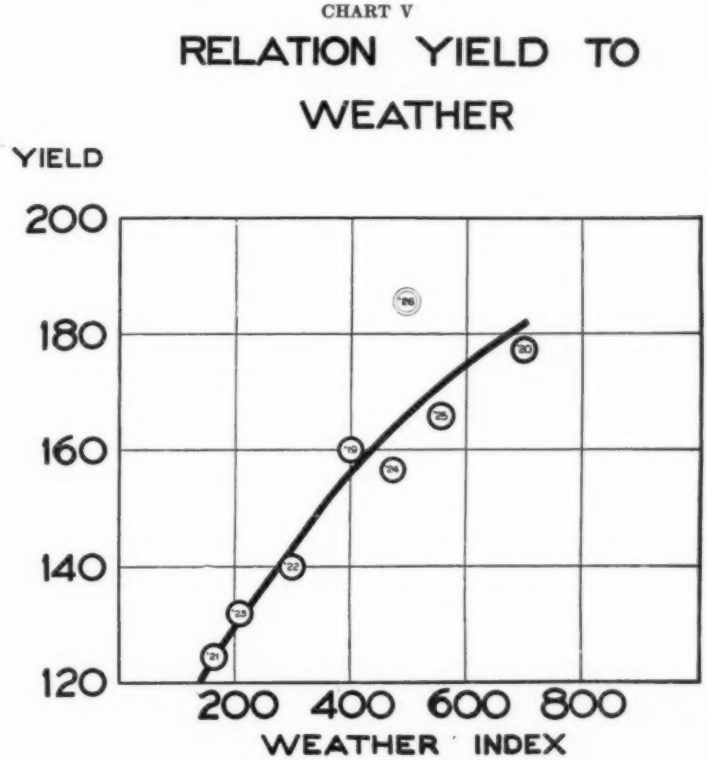
The method of forecasting yield per acre was an accidental discovery in an attempt to find some relation between coldness in winter and boll weevil damage in the following season. The study covered temperatures from the first of November until the last of March for the years since 1919. Records from three weather stations in the Northwest belt were used to measure cold waves as they came into the cotton belt, and records from three weather stations in the Eastern belt were used to measure how completely the cold waves crossed the belt. The data used were minimum daily temperatures. More weight was given to low temperatures than high. Temperatures above 32° F. were given a weight of zero since such temperatures could work little injury to hibernating weevils. Low temperatures for a number of consecutive days, however, were given more weight than low temperatures interspersed with high temperatures, since the former meant greater penetration. The index was constructed by summing the daily temperatures so weighted for the five winter months giving an aggregate. The aggregate for each year for each station was expressed as a relative of the average aggregate for that station during the six years, 1919–1924. The annual index for the cotton belt as a whole was then obtained by adding together the relatives for the six stations for the given year. The index constructed, then, is a measure of the amount and concentration of coldness in the cotton belt during the winter. When this index is compared with the average yield per acre in the following season the relationship revealed is strikingly close.

Chart V shows the observations for the eight years covered and the line drawn in generalizes the relationship between the two variables. If, on the basis of the curve and the winter weather index numbers the yield per acre is estimated and charted, together with the actual yield per acre as it subsequently proved to be the picture would look like Chart VI. The closeness of the two curves suggests the usefulness of this type of index in forecasting the average United States yield per acre before the crop is even planted.

On the other hand, I do not feel that a really satisfactory hypothesis as to why the winter temperature index should coincide so closely with the yields in the following year has been advanced. Until such an

hypothesis backed by scientific knowledge of causal relationships is forthcoming, therefore, this relationship must be regarded as something of a curiosity or novelty, and must be used with all due respect to the latent possibilities in novelties for producing the unexpected. In 1926, for example, there is quite a difference between the forecasted and the most recent estimate of the yield per acre.

CHART V



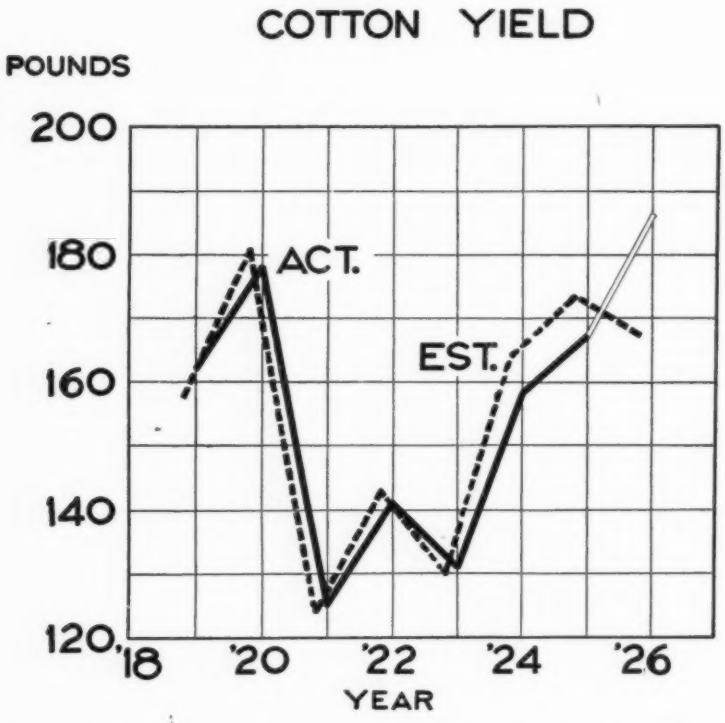
By the employment of forecasts of acreage changes and yields per acre an estimate of the size of the coming crop can be secured which can be used in making a forecast of the price about a year in advance by means of the relationships I first discussed. There was one other series employed in making the price forecast, however, which I have not described—namely the forecasts of the level of industrial activity. It is this method of forecasting the level of industrial activity a year in advance which will be taken up next.

The particular measure of industrial activity which was chosen was the average percentage of blast furnaces in blast for the twelve months

following the date the forecast is made. This index has contours so similar to the Standard Statistics Index of Industrial Activity, the Harvard Index of Manufacture, and the American Telephone and Telegraph Index of Business, that the forecasting of one is for most purposes essentially the forecasting of the others.

The first factor by means of which the average percentage of furnaces in blast for the following twelve months was forecasted was, as in the

CHART VI



other studies, a trend factor taken as the last two digits of the calendar year designation.

The second factor was the percentage that short term interest rates during the month preceding the date of the forecast were of bond yields during the same month. A high interest rate is apt to depress future business and vice versa.

The third factor was a series which gave a numerical description of the history of changes in interest rates during the two years preceding the date of the forecast. If interest rates have been steadily falling, even though they yet remain high a different effect upon subsequent

business is to be expected than if they had been steadily rising. Accordingly the average percentage change in the interest rate during the two years preceding the date of the forecast was computed. This was computed by finding the rates of change for every month in the period and averaging them. The rate of change for a given month was defined as the difference between the interest rate in the following month and in the preceding month taken as a percentage of the interest rate for the given month. In averaging these rates of change for the twenty-four months preceding the date of the forecast the rates of changes for the different months were given different weights. The most recent months were given large weights and the more remote months small weights. The measurement finally assembled may be termed a weighted average of percentage changes in short-time interest rates for the two years preceding the date of the forecast.

An analogous measurement was constructed for commodity prices from the Bureau of Labor Statistics index of all commodities. This measurement is essentially the one employed by Irving Fisher in his article appearing in this JOURNAL in 1925, "Our Unstable Dollar and the So-Called Business Cycle." This measurement may be termed the weighted average percentage change in prices during the two years preceding the date the forecast is made.

The last measurement used in forecasting the average percentage of furnaces in blast for the twelve months following the date of the forecast was the average percentage of furnaces in blast for the twelve months preceding the date of the forecast. It is almost inescapable that business conditions in any given year will bear some relation to business conditions in the preceding year.

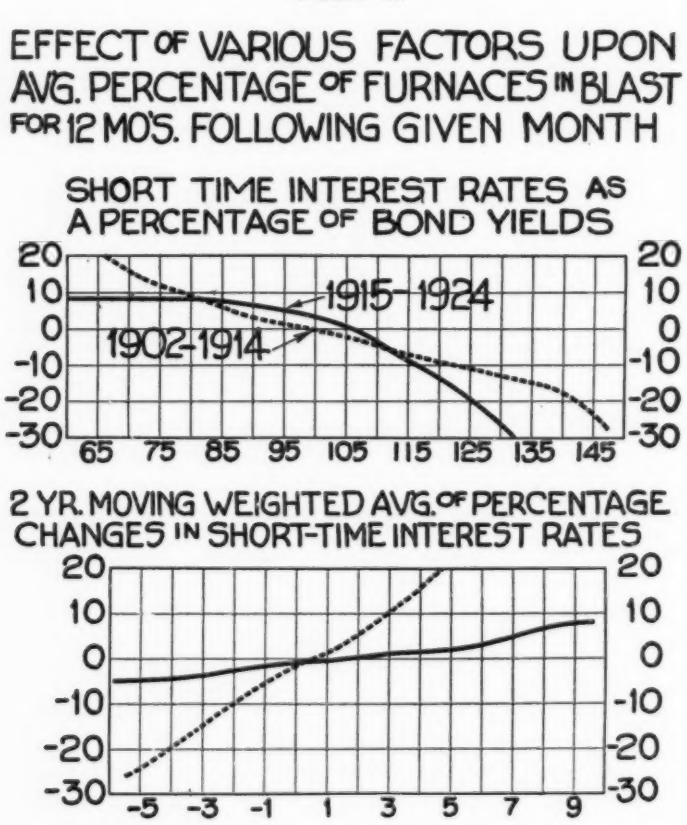
Supposing that a forecast were made on January 1 on the basis of the factors described, on February 1 there would be one more month's data and a forecast of the twelve months following February 1 could be made. As a matter of fact in setting up the data the observations for every month in the period 1902 to date were employed. Hence the thing being forecasted is essentially a moving average of the percentage of furnaces in blast for the twelve months following the date of the forecast.

In determining the relationships between the dependent and the independent factors described, it was found that a fairly definite change had appeared in those relationships about 1914. Hence two sets of relationships were determined, one for the period prior to 1914, the other for the period since that time. These relationships, or net regression curves, to give them their proper name, are shown in Chart VII.

The first of the graphs shows the net relationship of the short-time

interest rates as a percentage of bond yields to the average percentage of furnaces in blast for the next twelve months. The dotted line represents the relationship in the early period, the solid line the relation in the later period. It is interesting to note that nowadays not nearly so high an interest rate is required to bring about a given depressive result in business. Thus, today an interest rate 125 per cent of bond

CHART VII


EFFECT OF VARIOUS FACTORS UPON AVG. PERCENTAGE OF FURNACES IN BLAST FOR 12 MO'S. FOLLOWING GIVEN MONTH
SHORT TIME INTEREST RATES AS A PERCENTAGE OF BOND YIELDS
20
10
0
-10
-20
-30
1915-1924
1902-1914
65 75 85 95 105 115 125 135 145
2 YR. MOVING WEIGHTED AVG. OF PERCENTAGE CHANGES IN SHORT-TIME INTEREST RATES
-5 -3 -1 1 3 5 7 9

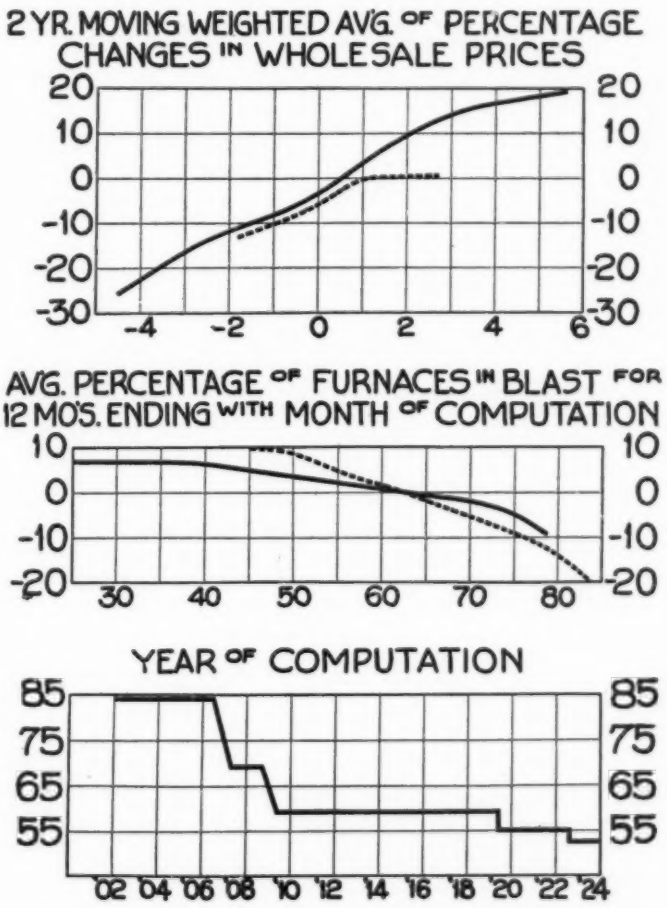
yields gives the same depressive influence of −.20 per cent as an interest rate of 137 per cent of bond yields did in the early period.

The next curves have to do with the relationship between the average rate of change in interest rates during a two year period and the average percentage of furnaces in blast in the ensuing twelve months. It is interesting to note that rising interest rates are not the herald of depressed business—although it is to be remembered that high interest rates are. Perhaps the explanation is that as long as interest rates rise

without getting to high levels, it is merely a manifestation of the fuller employment of capital.

The graph representing the relationship between the rate of change in commodity prices and subsequent business conditions proves to be

CHART VII (*Continued*)



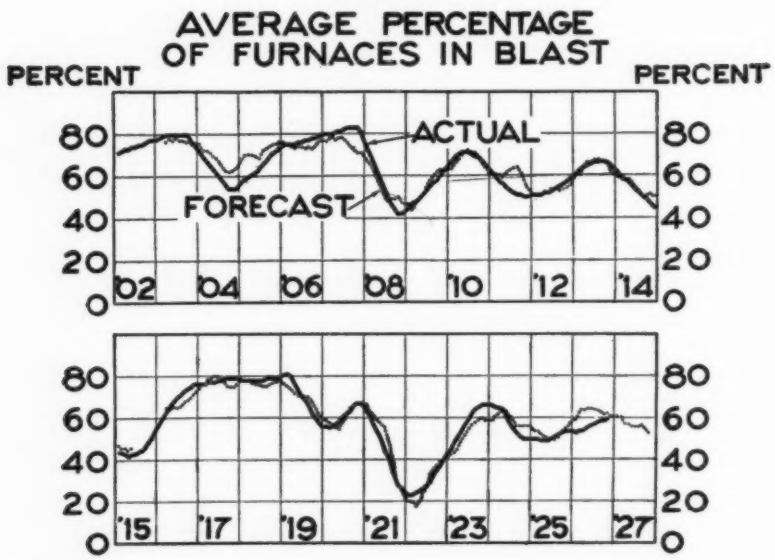
what would be expected: Rising prices mean greater business activity in the following year.

The graph representing the relationship between the average percentage of furnaces in blast during the last twelve months to the average percentage of furnaces in blast during the next twelve months is as would be expected: High levels in one year have a depressive effect upon the subsequent year.

The last curve represents the net trend in the percentage of furnaces in blast, the other factors being held constant. It is noteworthy chiefly because of the sharp decline following the drop in business of 1908.

If on the basis of these relationships forecasts were made each month over the past period—forecasts of the average percentage of furnaces in blast for the twelve months following the dates of the forecasts—and these forecasts were compared with the actual average percentages as they proved to be, the picture would look like Chart VIII. In this

CHART VIII



graph any point on either of the curves represents the average percentage of the furnaces in blast for the twelve months ending with the indicated month—the dotted curve the forecast, the black curve the actual. Note that the forecast curve indicates faithfully the depression of 1908, the fluctuations that took place during the World War, the severe depression immediately following the War and the subsequent fluctuations. A period offering a more rigorous test of such relations could hardly be found. For some reason, I haven't discovered exactly why, the forecast would have been misleading in 1911. It was these forecast items which were employed in the cotton price forecasting study which I first described. This then completes the description of the various statistical data employed in the forecasting of the price of cotton.

Before closing I should say a word or two about the statistical technique employed in determining the various relationships I have de-

scribed. This technique is that of multiple curvilinear correlation, which method tremendously broadens the analytical possibilities open to statisticians. But at the same time these methods must be employed with a full realization of the limitations involved and the assumptions implied in the multiple regression equation, or else one is apt to find himself with a "high-powered" absurdity as the result of his analyses. Both from the statistical and the economic point of view it is well to remember that there are definite restrictions upon the use of this technique for forecasting.

From the statistical point of view the methods employed assume—and it is an assumption fraught with peril to the unwary statistician—that the functional contribution of each independent to the variability in the dependent is independent of the values of other factors. This is not necessarily the case; in fact joint relationships are easily conceivable. Thus the effect of a high interest rate may be greater when business is at high levels than when it is at low, other things being equal. Methods of solving for joint relationships, though being developed, are yet in their infancy.

From the economic point of view there are also certain restrictions in forecasting by this technique to be observed. In the first place the particular phenomena which have been given statistical expression in a certain way may fail to be reflected in that statistical measurement. Thus the interest rate in itself and by itself is not so much of a factor, it is rather the condition of supply and demand for capital which is indexed by the interest rate that is important. And back of this condition are the growth of the population, the success of crops, the adjustment of human enterprise to human needs. Each of the factors selected in the forecasting studies may thus be considered as symbolic of fact rather than as a significant fact itself. If they should fail to reflect the facts, or come to represent different facts, then the validity of this analysis for forecasting is diminished.

The second economic consideration tending to invalidate the use of this type of analysis for forecasting is that in human affairs all things change. The relationships discovered may change. One such change was noted in the blast furnace forecasting study. This possibility of change in relationship is perhaps the most fundamental weakness in all statistical forecasting. It means that there is a large field for individual judgment. On the other hand this weakness in statistical forecasting is a weakness shared by all forecasting whether it be statistical or non-statistical in nature. For the only basis of forecasting the future is past experience; and if past experience fails to be indicative of future experience, not only the purely statistical, but all other scientific forecasting is deprived of its very foundation.

ON A LIMITATION IN THE APPLICABILITY OF THE CONTINGENCY COEFFICIENT

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Suppose two characters x and y both of which may be measured or described for each of N individuals. Some measure of the relationship between them is required. When both are measurable on a quantitative scale the correlation coefficient, r_{xy}, or the correlation ratio, ${}_x\eta_y$, ${}_y\eta_x$, serves the purpose admirably. Since the latter, η, depends solely on the constants of the arrays of one of the two variables, it may also be used in cases in which one of the two variables is not quantitatively measurable but is classed in categories. When both x and y must be treated as a number of non-quantitative categories, contingency methods[2] must be employed. Pearson has shown[3] that, with certain reservations concerning fineness of subdivision in classification, the first coefficient of contingency, where ϕ^2 is a value to

$$C_1=\sqrt{\frac{\phi^2}{1+\phi^2}},$$

be defined presently, is essentially identical with the product moment correlation coefficient as deduced from a normal correlation surface.

The value of the contingency coefficient is independent of the order of arrangement of the classes of x and y. It is, therefore, a convenient measure of the relationship between the two variables when these are measured in categories, the numerical equivalents of which on a quantitative scale may be entirely unknown.

The purpose of this article is to indicate that in certain cases in which the two variables, x and y, may be measured on a quantitative scale, the contingency coefficient will take a misleading value. If this be true for the contingency coefficient as determined when x and y have quantitative values, it may reasonably be assumed that in similar cases in which the contingency coefficient must be based upon the classification of the two variables in categories, it will again take a misleading value.

[1] Research Scholar, Farrer Memorial Trust of Australia.

[2] K. Pearson, "On the Theory of Contingency and its Relation to Association and Normal Correlation," *Drapers' Company Research Memoirs, Biometric Series*, 1, London, 1904.

[3] K. Pearson, *loc. cit.* pp. 7–9, 1904.

Two methods of obtaining an approximate value of the contingency coefficient in such cases are given.

While in practical statistical work it is rarely desirable to determine a contingency coefficient when the correlation coefficient, r, or the correlation ratio, η, can be determined, it is desirable in testing the contingency coefficient to determine this value from correlation surfaces in which both x and y are quantitatively measured and to compare it with one of the other more standard criteria of association. This course will be followed here.

Let $x_1, x_2, x_3, \ldots x_n, \ldots x_p$ be classes of the variable x, and $y_1, y_2, y_3, \ldots y_\nu, \ldots y_q$ be values of the second variable, y. The frequencies of these classes in the sample are $n_{x_1}, n_{x_2}, n_{x_3}, \ldots n_{x_p}$ for the variable x; and $n_{y_1}, n_{y_2}, n_{y_3}, \ldots n_{y_q}$ for the variable y; the total frequency of the sample being $\Sigma(n_x) = \Sigma(n_y) = N$.

Remembering that these two variables occur in association in the same individual (or separately in individuals which may be paired) the characteristics of any individual or pair of individuals may thus be $x_1y_1, x_1y_2, x_1y_3, \ldots x_1y_q, x_2y_1, x_2y_2, x_2y_3, \ldots x_2y_q, \ldots x_ny_\nu, \ldots x_py_q$. These occur in the frequencies $n_{x_1y_1}$, etc. If any class value of x be designated as the nth and any class value of y as the νth, the total number of individuals in the sample is $\Sigma(n_{x_ny_\nu}) = \Sigma(n_x) = \Sigma(n_y) = N$. Here the subscripts $1, 2, 3, \ldots n, \ldots p$, and $1, 2, 3, \ldots \nu, \ldots q$ serve merely to distinguish the several classes of x and y, which classes may or may not have numerical magnitudes or be arranged in scale order. When the subscripts distinguish between categories only,

$$\phi^2 = \chi^2/N = S\left[\frac{(n_{x_ny_\nu} - n'_{x_ny_\nu})^2}{n'_{x_ny_\nu}}\right]/N,$$

where $n'_{x_ny_\nu}$ is the theoretical frequency defined by the equation

$$n'_{x_ny_\nu} = N \cdot \frac{n_{x_n}}{N} \cdot \frac{n_{y_\nu}}{N} = \frac{n_{x_n} n_{y_\nu}}{N},$$

may be employed to lead to C_1, the first coefficient of contingency, to replace the correlation coefficient, r, or the correlation ratio, η.

Now let us consider the contingency surface presented as Table I, assuming that (i) the subscripts distinguish between numerical values of both x and y arranged in sequence on a uniform scale, and that (ii) identical subscripts denote actually identical numerical class values of the two variables. These two assumptions are realized in the case of the ordinary correlation surface for two quantitatively measurable

variables where the variables are measured on the same numerical scale from the same origin. Such cases are normally treated by product moment correlation, or in the case of non-linear regression by correlation ratio methods. Their use here is merely for the purpose of the illustration of principles. Let us further assume (iii) that in the case

TABLE I

CONVENTIONAL CONTINGENCY SURFACE FOR THE RELATIONSHIP BETWEEN X AND Y

	n_{x_1}	n_{x_2}	n_{x_3}	.....	n_{x_p}	N
y_q	$n_{x_1y_q}$	$n_{x_2y_q}$	$n_{x_3y_q}$	.....	$n_{x_py_q}$	n_{y_q}
⋮	⋮	⋮	⋮		⋮	⋮
y_3	$n_{x_1y_3}$	$n_{x_2y_3}$	$n_{x_3y_3}$	.....	$n_{x_py_3}$	n_{y_3}
y_2	$n_{x_1y_2}$	$n_{x_2y_2}$	$n_{x_3y_2}$	.....	$n_{x_py_2}$	n_{y_2}
y_1	$n_{x_1y_1}$	$n_{x_2y_1}$	$n_{x_3y_1}$	.....	$n_{x_py_1}$	n_{y_1}
	x_1	x_2	x_3	.....	x_p	

at hand the range of x is 1 to p, that of y is 1 to q, and $p=q$. If we make the further assumption (iv) that in any individual case of x_ny_r the value of x is a limiting value for y, the correlation or contingency surface becomes that illustrated in Table II. Such cases are not infrequent in statistics. For example, in determining the correlation between the number of children born and the number dying at or before

any specified age (say two years), the number of children dying can never exceed the number of children born.

In the general case (Table I) there are pq degrees of freedom for the distribution of the N variates, and the contingency between x and y is calculated on the assumption that individuals may fall into any one

TABLE II

CONTINGENCY SURFACE ON THE ASSUMPTION THAT X IS THE LIMITING VALUE OF Y

	n_{x_1}	n_{x_2}	n_{x_3}	.....	n_{x_p}	N
y_q	——	——	——	- - -	$n_{x_p y_q}$	n_{y_q}
. .	. .	. .	. .		. .	. .
y_3	——	——	$n_{x_3 y_3}$	.....	$n_{x_p y_3}$	n_{y_3}
y_2	——	$n_{x_2 y_2}$	$n_{x_3 y_2}$	.....	$n_{x_p y_2}$	n_{y_2}
y_1	$n_{x_1 y_1}$	$n_{x_2 y_1}$	$n_{x_3 y_1}$	.....	$n_{x_p y_1}$	n_{y_1}
	x_1	x_2	x_3	.....	x_p	

of the pq compartments of the contingency surface, where they occur in the theoretical frequency of $n'_{x_n y_y}$, the deviations of the actual numbers of observations from these theoretical frequencies, $n_{x_n y_y} - n'_{x_n y_y}$, arising from (a) errors of random sampling and (b) the existence of correlation (or contingency) between x and y.

In the second scheme[1] (Table II), in which $p=q$ and x is a limiting value of y, the compartments into which individuals may fall are $p+\frac{1}{2}(pq-p)=q+\frac{1}{2}(pq-q)$ in number.

It is evident that under the limitations specified $pq-[p+\frac{1}{2}\ (pq-p)]=pq-[q+\frac{1}{2}\ (pq-q)]=\frac{1}{2}\ (pq-p)=\frac{1}{2}\ (pq-q)$ of the compartments of the contingency surface must of necessity lack observational frequencies. Nevertheless they contribute to the value of χ^2, and so to a spurious magnitude of the contingency coefficient.

The total of the probabilities of the cells which may contain frequencies is

$$\alpha=p_{x_1y_1}+p_{x_2y_1}+p_{x_2y_2}+p_{x_3y_1}+\cdots+p_{x_py_q}.$$

Likewise the total of the probabilities of the cells which cannot contain actual frequencies is

$$\beta=p_{x_1y_2}+p_{x_1y_3}+\cdots+p_{x_1y_q}+p_{x_2y_3}+p_{x_2y_4}+\cdots+p_{x_2y_q}+\cdots+p_{x_{p-1}y_q}.$$

The total contribution to χ^2 of the cells which may contain frequencies will be

$$\gamma=\frac{(n_{x_1y_1}-n'_{x_1y_1})^2}{n'_{x_1y_1}}+\frac{(n_{x_2y_1}-n'_{x_2y_1})^2}{n'_{x_2y_1}}+\frac{(n_{x_2y_2}-n'_{x_2y_2})^2}{n'_{x_2y_2}}+\frac{(n_{x_3y_1}-n'_{x_3y_1})^2}{n'_{x_3y_1}}+\cdots+\frac{(n_{x_py_q}-n'_{x_py_q})^2}{n'_{x_py_q}}.$$

The contribution to χ^2 of cells which for physical reasons cannot contain frequencies will be

$$\frac{(-n'_{x_1y_2})^2}{n'_{x_1y_2}}=n'_{x_1y_2},\quad \frac{(-n'_{x_1y_3})^2}{n'_{x_1y_3}}=n'_{x_1y_3},\text{ etc.},$$

or a total of

$$\delta=n'_{x_1y_2}+n'_{x_1y_3}+\cdots+n'_{x_1y_q}+n'_{x_2y_3}+n'_{x_2y_4}+\cdots+n'_{x_{p-1}y_q}.$$

We illustrate by Table V showing the relationship between the number of ovules formed and the number of seeds matured in 28,554 pods of *Cercis*.[2]

The probabilities of number of ovules per pod for each class of

[1] The theoretical limits $x_1=y_1$, $xp=y_q$, need not in all cases be actually needed to make the reasoning valid, but the case can be more readily discussed on these assumptions. The only essential is that x be the limiting value of y.

[2] J. Arthur Harris, "On the Relationship Between the Number of Ovules Formed and the Number of Seeds Developing in Cercis." *Bulletin Torrey Botanical Club.* 40: 243–256, 1914.

pods, p_x, are given in the first column of constants in Table III, while the comparable probabilities for number of seeds per pod, p_y, are similarly given in Table IV. These tables also contain adjusted probabilities, p'_x, p''_x, p'''_x, p'_y, p''_y, p'''_y, to be discussed later.

For convenience of comparison with adjusted probabilities to be

TABLE III
FREQUENCIES AND PROBABILITIES OF X

Ovules per pod X	n_x	P_x	P'_x	P''_x	P'''_x
1	7	0.00024515	0.00000688	0.00024515	0.00000536
2	348	0.01218742	0.01201173	0.01218743	0.00162822
3	2434	0.08524201	0.03033730	0.08524200	0.03291107
4	9618	0.33683547	0.29595865	0.33683548	0.25252013
5	9996	0.35007353	0.39746436	0.35007354	0.39105268
6	5289	0.18522799	0.22692109	0.18522798	0.26033577
7	826	0.02892766	0.03574197	0.02892764	0.05018425
8	36	0.00126077	0.00155803	0.00126077	0.00236232
Total	28554	1.00000000	1.00000001	0.99999999	1.00000000

TABLE IV
FREQUENCIES AND PROBABILITIES OF Y

Seeds per pod y	n_y	P_y	P'_y	P''_y	P'''_y
1	648	0.02269383	0.02804461	0.03169126	0.02269384
2	3166	0.11087763	0.13698685	0.13363955	0.11087764
3	7120	0.24935210	0.30431344	0.32276754	0.24935210
4	9368	0.32808012	0.36583434	0.35164166	0.32808013
5	5932	0.20774673	0.14517788	0.12424708	0.20774673
6	2073	0.07259930	0.01932647	0.01575678	0.07259929
7	242	0.00847518	0.00031617	0.00025590	0.00847517
8	5	0.00017511	0.00000027	0.00000022	0.00017511
Total	28554	1.00000000	1.00000003	0.99999999	1.00000001

TABLE V
CONVENTIONAL CONTINGENCY SURFACE FOR 28,554 PODS OF *CERCIS*

Total	7	348	2434	9618	9996	5289	826	36	28554
Y = 8 P_y = 0.00017511	n = 0 n' = 0.00122577 n-n' = -0.0012 c = 0.0012	0 0.06093826 -0.0609 0.0609	0 0.42621776 -0.4262 0.4262	0 1.68420798 -1.6842 1.6842	0 1.75039955 -1.7504 1.7504	0 0.92615679 -0.9262 0.9262	0 0.14464089 -0.1446 0.1446	5 0.00630395 4.9937 3955.7800	5 5.00009095
Y = 7 P_y = 0.00847517	n = 0 n' = 0.05932630 n-n' = -0.0593 c = 0.0593	0 2.94935811 -2.9494 2.9494	0 20.6285644 -20.6286 20.6286	0 81.5141851 -81.5142 81.5142	0 84.7177981 -84.7178 84.7178	0 44.8251744 -44.8252 44.8252	232 7.00049141 224.9995 7231.6031	10 0.30510634 9.6949 308.0601	242 242.0000041
Y = 6 P_y = 0.07259929	n = 0 n' = 0.50819598 n-n' = -0.5082 c 0.5082	0 25.2645439 -25.2645 25.2645	0 176.706677 -176.7067 176.7067	0 698.259972 698.2600 698.2600	0 725.702493 -725.7025 725.7025	1806 383.977647 1422.0234 5266.3162	261 59.9870221 201.0330 673.9415	6 2.61357637 3.3864 4.3877	2073 2073.000127
Y = 5 P_y = 0.20774673	n = 0 n' = 1.45422984 n-n' = -1.4542 c = 1.4542	0 72.2958363 -72.2958 72.2958	0 505.655555 -505.6556 505.6556	0 1998.10805 -1998.1081 1998.1081	3955 2076.63628 1878.3637 1699.0217	1787 1098.77246 688.2275 431.0784	190 171.598824 8.4021 0.4113	10 7.47888781 2.5211 0.8499	5932 5932.000128
Y = 4 P_y = 0.32808013	n = 0 n' = 2.29656521 n-n' = -2.2966 c 2.2966	0 114.171845 -114.1718 114.1718	0 798.547059 -798.5471 798.5471	4582 3155.47469 1426.5253 644.9028	3630 3279.48893 350.5111 37.4626	1064 1735.21582 -671.2158 259.6396	88 270.994226 -182.9942 123.5704	4 11.8108934 -7.8109 5.1656	9368 9368.000032
Y = 3 P_y = 0.24935210	n = 0 n' = 1.74546796 n-n' = -1.7455 c 1.7455	0 86.7744999 -86.7745 86.7745	1407 606.923028 800.0770 1054.7024	3536 2398.26850 1137.7315 539.7365	1691 2492.52356 801.5236 257.7468	444 1318.82326 -874.8233 580.3020	42 205.964864 -163.9649 130.5295	0 8.97668222 -8.9767 8.9767	7120 7119.999863
Y = 2 P_y = 0.11087704	n = 0 n' = 0.77614492 n-n' = -0.7761 c 0.7761	281 38.5854050 242.4146 1522.9810	837 269.876183 567.1238 1191.7666	1264 1066.42114 197.5789 36.6060	620 1108.33287 -488.3329 215.1601	142 585.431841 -444.4318 336.8160	21 91.5849437 -70.5849 54.4001	1 3.99159798 -2.9916 2.2421	3166 3166.000132
Y = 1 P_y = 0.02269384	n = 7 n' = 0.15885718 n-n' = 6.8411 c 294.6083	67 7.89745350 59.1025 422.3078	190 55.2368081 134.7632 328.7866	236 218.269353 17.7306 1.4403	100 226.847621 -126.8476 70.9301	46 120.027720 74.0277 45.6570	2 18.7451145 -16.7451 14.9585	0 0.81697385 -0.8170 0.8170	648 647.9999074
	X = 1 P_x = 0.00024515	2 0.01218743	3 0.08524200	4 0.33683547	5 0.35007354	6 0.18522799	7 0.02892765	8 0.00126077	Total

used later, the probabilities $p_{z_n y_y}$ computed from the probabilities p_x, p_y, of Tables III and IV are given as the first entries underneath the frequencies in Table VI.

In the several cells of the surface represented as Table V, n denotes the actual frequencies of pods with given numbers of ovules and seeds as shown by the marginal entries, n' denotes the computed frequencies

TABLE VI

FREQUENCIES AND PROBABILITIES OF $X_n Y_y$ IN 25,554 PODS OF *CERCIS*

Total		7	6	5	4	3	2	1	Total
Y=8	n =	0	3	2434	9519	9996	5289	826	36	28554	5
	P =	0.00000004	0.00000213	0.00001493	0.00005898	0.00006130	0.00003244	0.00000507	0.00000022		
	P' =								0.00000027		
	P'' =								0.00000022		
Y=7	n =	0	0	0	0	0	0	232	10	242	
	P =	0.00000208	0.00010329	0.00072244	0.00285474	0.00296693	0.00156984	0.00024517	0.00001069		
	P' =							0.00030297	0.00001320		
	P'' =							0.00024521	0.00001069		
Y=6	n =	0	0	0	0	0	1906	261	6	2073	
	P =	0.00001780	0.00088480	0.00618851	0.02445402	0.02541509	0.01344742	0.00210013	0.00009153		
	P' =						0.01660903	0.00259530	0.00011311		
	P'' =						0.01356476	0.00210049	0.00009163		
Y=5	n =	0	0	0	0	3955	1787	180	10	5932	
	P =	0.00005093	0.00253190	0.01770375	0.06997647	0.07272663	0.03848051	0.00600962	0.00026192		
	P' =					0.08987416	0.04755346	0.00742658	0.00032368		
	P'' =					0.07916520	0.03881628	0.00601068	0.00026192		
Y=4	n =	0	0	0	4528	3630	1064	89	4	9368	
	P =	0.00006043	0.00299845	0.02796621	0.11050902	0.00485217	0.06076962	0.00949059	0.00041363		
	P' =				0.13656460	0.14193208	0.07509792	0.01172823	0.00051116		
	P'' =				0.15542670	0.12500911	0.06129998	0.00949225	0.00041363		
Y=3	n =	0	0	1407	3536	1691	444	42	0	7120	
	P =	0.00006115	0.00303896	0.02125527	0.08399063	0.03729157	0.04618699	0.00721317	0.00031438		
	P' =			0.02826685	0.10378399	0.10787322	0.05707698	0.00891390	0.00038850		
	P'' =			0.05550787	0.11812967	0.09501119	0.04659000	0.00721443	0.00031438		
Y=2	n =	0	261	837	1284	620	142	21	1	3166	
	P =	0.00002718	0.00138131	0.00945143	0.03734752	0.03881833	0.02053704	0.00320743	0.00013979		
	P' =		0.00166993	0.00167990	0.04615334	0.04799723	0.02538002	0.00396368	0.00017275		
	P'' =		0.01011679	0.02468229	0.05552789	0.04224796	0.02071685	0.00320799	0.00013979		
Y=1	n =	7	67	190	236	100	45	2	0	648	
	P =	0.00000556	0.00027658	0.00193447	0.00764409	0.00794451	0.00420353	0.00065648	0.00002861		
	P' =	0.00000698	0.00034179	0.00239058	0.00944642	0.00981767	0.00519485	0.00081126	0.00003536		
	P'' =	0.00024515	0.00207068	0.00505184	0.01075113	0.00864708	0.00424021	0.00065659	0.00002861		
	X =	1	2	3	4	5	6	7	8	Total	

as derived from the first of the three probabilities given in the cells of Table VI, $n-n'$ denotes the difference between the observed and the theoretical frequencies, while $c=(n-n')^2/n'$ is the contribution of the cell to χ^2.

Summing the values of c for the cells in which actual frequencies can occur and cannot occur, we have respectively

$$\gamma=17773.6623, \qquad \delta=5447.9554, \qquad \chi^2=23221.6177,$$
$$\phi^2=\chi^2/N=1.16346633, \qquad \phi^2/(1+\phi^2)=0.5377788.$$

Whence we deduce for comparison with r_{os} and ${}_o\eta_s$

$$r_{os}=0.6482\pm0.0023$$
$${}_o\eta_s=0.6484\pm0.0023$$
$$C_1=0.7333\pm\text{——}.$$

The value of C_1 is materially and probably significantly larger than r or η. This may reasonably be assumed to be due to the contribution to χ^2 of the cells which cannot contain frequencies, since these contribute 23.46 per cent of the total value of χ^2.

We have now to consider some modification of the contingency method which will eliminate this source of a spurious contribution to the value of χ^2 of which C_1 is a function.

A first requisite is obviously to concentrate all the theoretical frequencies into the cells of the contingency surface which may contain actual frequencies, maintaining a total frequency of N.

Two methods are suggested.

First Method.—We distribute the total of the probabilities occupying the triangle of the contingency surface in which actual frequencies cannot occur among the probabilities of the triangle of the surface in which they may occur in proportion to the values of the latter. Remembering that these summed probabilities are β and α we have merely to note that the values of n' for the area of the surface in which frequencies may occur must be increased by the factor $(\alpha+\beta)/\alpha=1/\alpha$. Thus the adjusted probabilities for any class, $x_n y_\nu$, are $p'_{x_n y_\nu}=\frac{1}{\alpha}p_{x_n y_\nu}$.

In the example given, $\alpha=0.809205189$.

Applying these methods of adjustment we obtain the values of p' given in the second row of values under the actual frequencies in Table VI. They lead to

$$\chi^2=21635.1681, \qquad C_1=0.6548\pm\text{——}.$$

C_1 differs from r_{xy} by $+0.0066$, and from ${}_x\eta_y$ by $+0.0064$.

Second Method.—The concentration of the probabilities of y into a

successively smaller number of cells for successively smaller values of x may be effected by distributing the total values of p_y for the compartments in which actual frequencies cannot occur because of physical limitations into the cells of the same array of y for a given value of x which may contain frequencies.

Remembering that the maximum range of an array of y is that for class x_p the frequency of which is represented in the sample by n_{x_p} and the probability of which is represented by $p_{x_p}=n_{x_p}/N$, we note that the probabilities of x_py_1, x_py_2, x_py_3, $\cdots$ x_py_q will require no adjustment since the values found by the ordinary contingency method may properly be used, *i.e.*, $p''_{x_py_1}=p_{x_py_1}$, etc. For the $(p-1)$th, $(p-2)$th, etc., classes of x the theoretical frequencies of y must be concentrated into $q-1$, $q-2$, etc., cells.

In concentrating the $(p-1)$th probability class of x into $(q-1)$ cells, it might at first thought seem proper to concentrate them merely in proportion to $q/(q-1)$, etc., but this is not valid, since the relation $S(p'_{x_ny_p})=1$ is not thus realized.

Let the number of classes of y associated with x_n be n, and let a presubscript x indicate that the value of y is associated with a specific value of x. The values of ${}_xp_{y_{(n+1)}}$ to ${}_xp_{y_q}$ must be concentrated into the classes ${}_xp_{y_1}$ to ${}_xp_{y_n}$. It seems most logical to concentrate them in proportion to the probabilities of p_{y_1} to p_{y_n}. This redistribution for the class x_n of the q probabilities of y leads to the n adjusted probabilities, $p''_{x_ny_1}$, $p''_{x_ny_2}$, $\cdots$, $p''_{x_ny_n}$, which satisfy the following relations.

$$p''_{x_ny_1}=p_{x_n}\frac{\overset{j=q}{\underset{j=1}{S}}(p_{y_j})}{\overset{j=n}{\underset{j=1}{S}}(p_{y_j})}p_{y_1}=p_{x_n}\frac{1}{\overset{j=n}{\underset{j=1}{S}}(p_{y_j})}p_{y_1},$$

$$p''_{x_ny_2}=p_{x_n}\frac{\overset{j=q}{\underset{j=1}{S}}(p_{y_j})}{\overset{j=n}{\underset{j=1}{S}}(p_{y_j})}p_{y_2}=p_{x_n}\frac{1}{\overset{j=n}{\underset{j=1}{S}}(p_{y_j})}p_{y_2},\text{ etc.}$$

These fulfill the necessary condition

$$S(p''_{x_ny_p})=S(p_{x_ny_p})=1,$$

where S denotes summation for the whole surface.

The theoretical frequency for any class is $Np''_{x_ny_p}$ and $S(Np''_{x_ny_p})=S(Np_{x_ny_p})=N$.

Illustrating by the series of data treated above we have the results for computed probabilities set forth as the third entry (p'') of values of $p_{x_n y_\nu}$ in Table VI. We deduce $\chi^2 = 21396.3908$ which leads to $C_1 = .6545$. This differs [1] from the value of C_1 determined by the first method of adjustment by $-.0003$, and from r by $+.0063$ and from η by $+.0061$.

In a true contingency surface the sum of the probabilities of the columns and rows of frequencies is identical with the probabilities of the classes of the original variables. The totals of the individual arrays of the adjusted probabilities of various combinations of x and y cannot be identical with the original probabilities of the two variables. In the case of the first method of adjustment neither p'_x nor p'_y is identical with the comparable value of p_x, p_y. In the case of the second method of adjustment the summed values of $p''_{x_n y_\nu}$ for any class of x are identical with p_x, but this cannot be true for the sums of $p''_{x_n y_\nu}$ for the arrays of y. The discrepancies between the sums of the adjusted and the original probabilities may be seen in the columns of constants of Tables III and IV.

Algebraically it is quite possible to concentrate the probabilities of x for given arrays of y in the like manner to that indicated in the determination of $p''_{x_n y_1}$, $p''_{x_n y_2}$, etc., above. Thus, remembering that $y \leqq x$, let n be the class value of x which is a minimum for the occurrence in association with x of the given class of y. Then the probabilities of $x_1, x_2, x_3, \ldots x_{n-1}$ must be concentrated into the classes $x_n, x_{n+1}, x_{n+2}, \ldots x_p$. Assuming that the summed probabilities $\underset{n-1}{\overset{1}{S}}({}_y p_x)$, where the prescript y denotes probabilities of x for a specific class value of y, should be distributed among the nth to the pth classes of x in proportion to the probabilities of the latter we write

$$p'''_{x_n y_n} = p_{y_n} \frac{\overset{j=p}{\underset{j=1}{S}}(p_{x_j})}{\overset{j=p}{\underset{j=n}{S}}(p_{x_j})} p_{x_n} = p_{y_n} \frac{1}{\overset{j=p}{\underset{j=n}{S}}(p_{x_j})} p_{x_n},$$

$$p'''_{x_{(n+1)} y_n} = p_{y_n} \frac{\overset{j=p}{\underset{j=1}{S}}(p_{x_j})}{\overset{j=p}{\underset{j=n}{S}}(p_{x_j})} p_{x_{(n+1)}} = p_{y_n} \frac{1}{\overset{j=p}{\underset{j=n}{S}}(p_{x_j})} p_{x_{(n+1)}}, \text{ etc.}$$

These *fulfill* the necessary conditions

$$S(p'''_{x_n y_\nu}) = S(p_{x_n y_\nu}) = 1,$$

[1] In the determining of all these constants a larger number of places were retained in the calculation than is indicated in the tables. Thus the coefficients are probably nearly correct in the fourth place.

and $S(Np'''_{x_n y_p}) = S(Np_{x_n y_p}) = N$, where S denotes summation for the entire surface. The disadvantage of the method is that while the totals of the adjusted values of $p'''_{x_n y_p}$, for the arrays of x associated with y are identical with the values of $p_{y_1}, p_{y_2}, p_{y_3}, \ldots p_{y_q}$, the totals of the adjusted values of $p'''_{x_n y_p}$ for the arrays of y associated with given values of x differ from the values of $p_{x_1}, p_{x_2}, p_{x_3}, \ldots p_{x_p}$. This is shown by the values p'''_x in Table III. Since x is the limiting value of y, and must be assumed to be the more fundamental or independent variable, the modification of the probabilities of x in the sample as a whole is even less legitimate than the modification of the probabilities of y.

The consequence of this method of adjustment is seen in the contingency coefficient computed in this way, which has the value $C_1 = 0.4693$ as compared with $r = 0.6482$ and $\eta = 0.6484$.

The failure of the summed probabilities of arrays to agree with the probabilities of the original classes of x and y is a fundamental objection to the two methods of adjustment suggested here, and serves to emphasize the limitation of the contingency method *as applied to this particular class of surface.*

While the formula for the probable error of the contingency coefficient has been given by Blakeman and Pearson,[1] it has not been determined here, since its sole use would be for testing the significance of the difference between the contingency coefficient and the correlation coefficient or the correlation ratio. The probable error of either of these differences cannot be determined until the correlation between the contingency coefficient and the correlation coefficient and the correlation ratio, respectively, are known. It is sufficient for our present purpose to note that in the present case at least there is reasonably close agreement between the contingency coefficient as determined by either of the modifications of the formula which we have suggested and the correlation coefficient as determined by the product moment method.

The present data have been given as an illustration of method only. Its use has in part been determined by the fact that a very large number of observations were available, thus justifying critical comparisons of the three methods.

It must be noted that the specific relationship here dealt with is one which requires for its full interpretation not merely the calculation of the correlation coefficient but the correlation between the independ-

[1] John Blakeman and Karl Pearson, "On the Probable Error of Mean-Square Contingency," *Biometrika*, 5, 191–197, 1906.

ent variable and the deviation of the dependent variable from its probable value,[1] for which the standard deviations and regression equation formula are also available.[2] These problems have been considered from the physiological side in a number of papers.

[1] J. Arthur Harris, "The Correlation Between a Variable and the Deviation of a Dependent Variable from Its Probable Value," *Biometrika*, 6, 438–443, 1909.

[2] J. Arthur Harris, "Further Illustrations of the Applicability of a Coefficient Measuring the Correlation Between a Variable and the Deviation of a Dependent Variable from Its Probable Value," *Genetics*, 3, 328–352, 1918.

BAR CHARTS VERSUS CIRCLE DIAGRAMS

By Frederick E. Croxton and Roy E. Stryker [1]
Columbia University

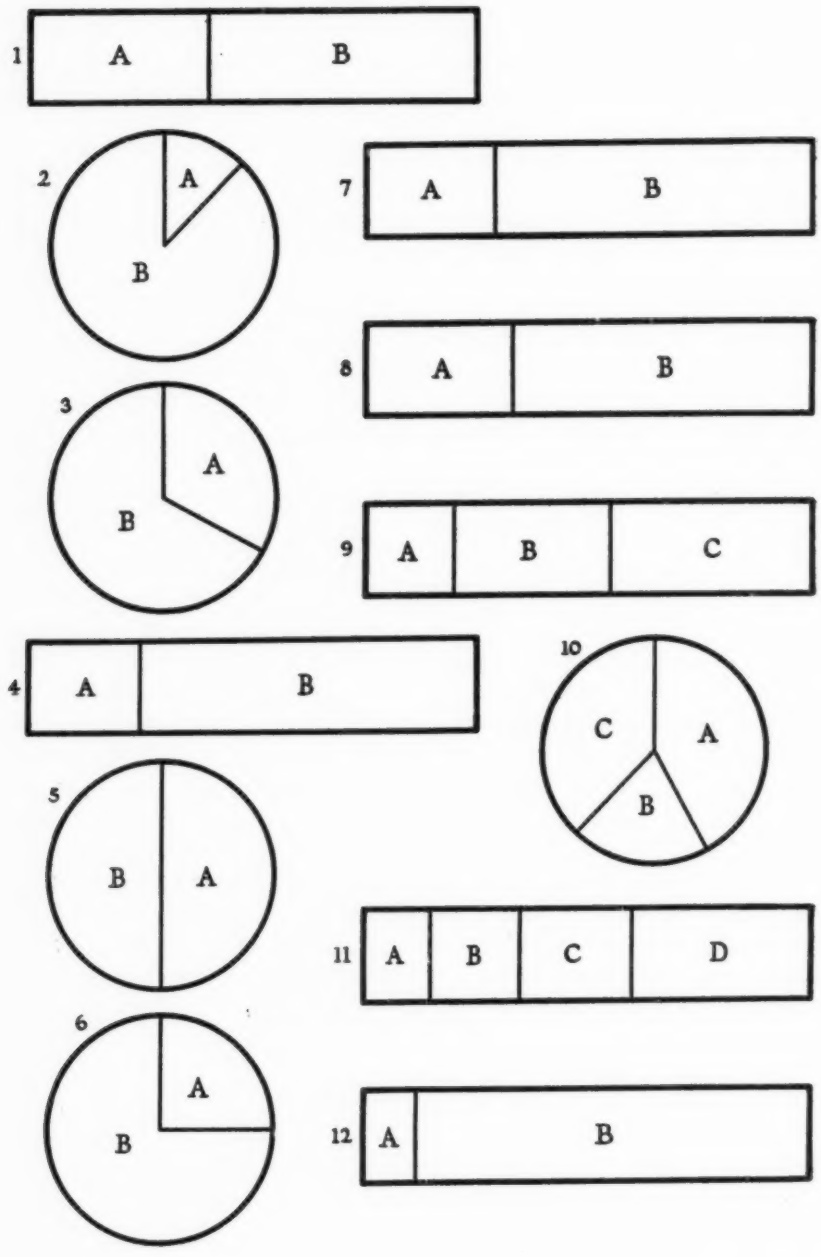
This study of bars and circles for presenting component parts was undertaken in an effort to arrive at a conclusive answer to the question of relative "accuracy of judgment" when based upon these two types of diagrams, which was raised by Eells in the June, 1926, issue of this Journal. In the March, 1927, issue one of the present writers presented some additional data concerning the accuracy of judgment of component part bars and circles. Neither of these two studies was conclusive; the first because of an insufficient number of observers; the second because of an insufficient number of diagrams. The present study, directed solely toward determining "accuracy of judgment," gives no attention to the other three points of "rapidity of judgment," "method of judgment," and "popularity and appeal" discussed by Eells.

The diagrams used numbered 27, as that proved to be the maximum number which a group of students could judge during a class period. The number of observers was 807. The 27 diagrams presented 13 different combinations of percentages; six of two parts, three of three parts, three of four parts, and one of five parts. For each combination one bar and one circle were constructed; in addition two different arrangements of the circle diagram showing 25 and 75 per cent were used (Figures 6 and 15). The diagrams, on individual cards 22 by 28 inches in size, were presented to the observers one card at a time in the order shown herewith, and it is believed that the observers did not discover the fact that there was a pair of diagrams for each combination of percentages. In order that the estimates should be based solely upon the diagrams and not upon scales, all diagrams were presented as shown in the accompanying figures, without scales.

TWO-PART DIAGRAMS

Six pairs of two-part diagrams were used, presenting the percentages 12 and 88, 25 and 75, 29 and 71, 33 and 67, 40 and 60, and 50 and 50. In addition, for the percentages 25 and 75, two different arrangements of the parts of the circle were compared (Figures 6 and 15).

[1] The writers wish to express their indebtedness to the many instructors at Columbia University, The College of the City of New York, and The American Institute of Banking who permitted their classes to be used in connection with this study.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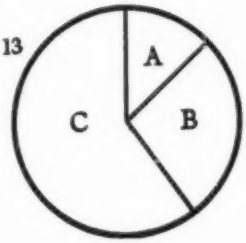

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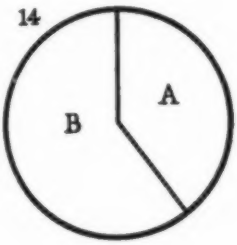

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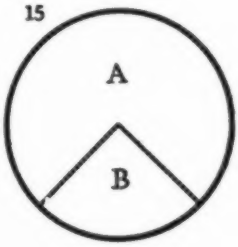

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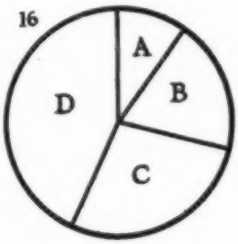

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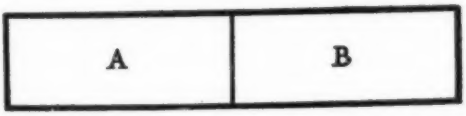

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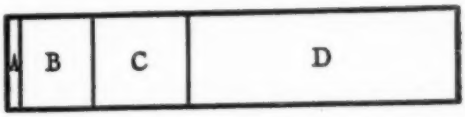

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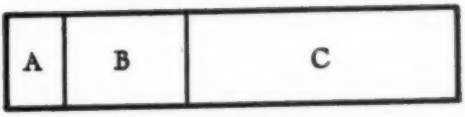

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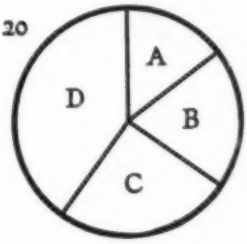

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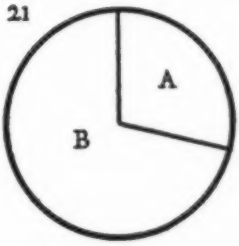

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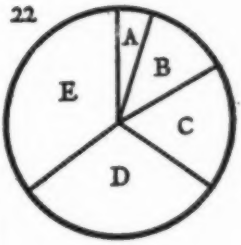


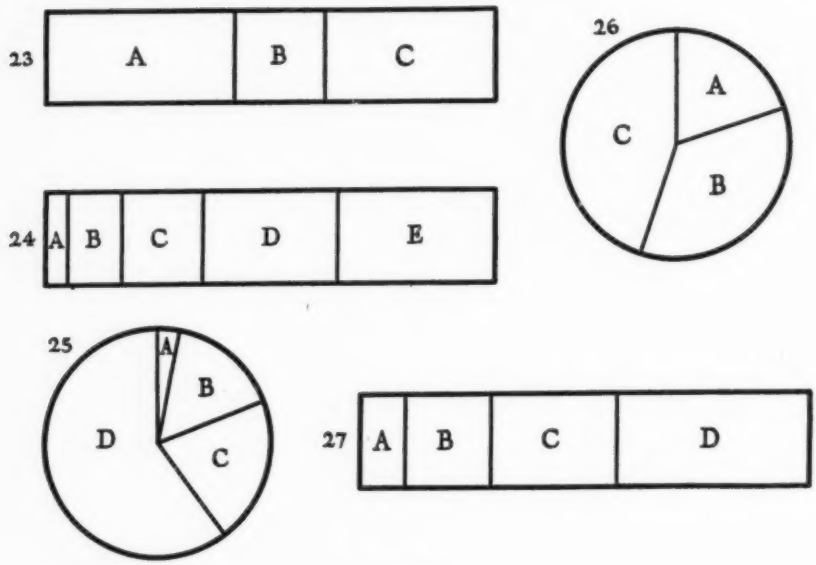


TABLE I

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 12 AND 88

Measures	Bar (Figure 12)	Circle (Figure 2)
Mean	4.20	5.25
Standard deviation	3.16	6.44
Standard error of mean	.11	.23

The bar (Figure 12) and the circle (Figure 2) were each divided into two parts to show the percentages 12 and 88. The mean error in judging the diagrams was 4.20 for the bar and 5.25 for the circle. The observed difference between the means was 1.05. This was 4.2 times the standard error of the difference between the two means, which was .25. In this case the difference between the mean errors appears to be significant, indicating that the bar is preferable to the circle.

TABLE II

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLES SHOWING PERCENTAGES 25 AND 75

Measures	Bar (Figure 4)	Circle (Figure 6)	Circle (Figure 15)
Mean	5.34	.58	3.47
Standard deviation	6.28	3.70	6.38
Standard error of mean	.22	.13	.22

Figure 4, a bar, and Figure 6, a circle, presented the percentages 25 and 75. The mean error in estimating the bar was 5.34 and in estimating the circle was .58. The observed difference, of 4.76, was 18.3 times the standard error of the difference between the means, which was .26. The circle of Figure 6 is unquestionably to be preferred to the bar of Figure 4.

In showing the percentages 25 and 75 by means of a pie diagram the question arose of the possibility that there might be some difference in the accuracy of judgment when based on a right-angle section centered at 1:30 o'clock, as in Figure 6, and when based on a right-angle section centered at 6 o'clock, as in Figure 15. The data of Table II indicate that the mean error is much less when the 25 per cent section is centered at 1:30 o'clock than when it is centered at 6 o'clock. This is at least partly due to the fact that, in the former case the right angle is more easily discerned than in the latter. The observed difference between the two means (2.89) was 11.1 times the standard error of the difference between the means (.26).

While the circle of Figure 15 proved undoubtedly less desirable than that of Figure 6, it nevertheless appears to have been judged more accurately than the bar of Figure 4. The mean error in judging the bar (Figure 4) was 5.34 and in judging the circle (Figure 15) was 3.47. The difference between the mean errors was 1.87, a value 6.0 times the standard error of the difference between the means, which was .31.

TABLE III

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 29 AND 71

Measures	Bar (Figure 7)	Circle (Figure 21)
Mean	6.82	3.76
Standard deviation	5.99	3.81
Standard error of mean	.21	.13

In the case of the diagrams showing percentages of 29 and 71 the advantage appears to lie with the circle. The mean error in estimating the circle of Figure 21 was 3.76, while the mean error in estimating the bar of Figure 7 was 6.82. The observed difference of 3.06 was 12.2 times the standard error of the difference between the means, which was .25.

The percentages 33 and 67 were judged more accurately when presented by means of a bar (Figure 8) than when shown by a pie diagram (Figure 3). The mean error in the case of the bar was 4.27 and in the

TABLE IV

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 33 AND 67

Measures	Bar (Figure 8)	Circle (Figure 3)
Mean	4.27	6.08
Standard deviation	5.20	6.29
Standard error of mean	.18	.22

case of the circle 6.08. The observed difference (1.81) was 6.4 times the standard error of the difference (.28).

TABLE V

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 40 AND 60

Measures	Bar (Figure 1)	Circle (Figure 14)
Mean	4.43	4.21
Standard deviation	7.39	5.30
Standard error of mean	.26	.19

For presenting the percentages 40 and 60 there is no choice between the bar and circle. In estimating the bar of Figure 1 a mean error of 4.43 was made, while for the circle of Figure 14 the mean error was 4.21. The observed difference (.22) was not significant, as it was but .7 times the standard error of the difference between the means (.32).

TABLE VI

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 50 AND 50

Measures	Bar (Figure 17)	Circle (Figure 5)
Mean	3.24	.61
Standard deviation	4.17	3.49
Standard error of mean	.15	.12

The figures of Table VI indicate that for the percentages 50 and 50 the accuracy of judgment was much greater when based upon the circle of Figure 5 than when based upon the bar of Figure 17, attributable, at least in part, to the fact that the 50-50 relationship on the circle is shown by a straight line. The mean error in judging the bar was 3.24 and in judging the circle .61. The observed difference of 2.63, was 13.8 times the standard error of the difference between the means (.19).

THREE-PART DIAGRAMS

Three pairs of three-part diagrams were included in the study. These showed the percentages 13, 27, and 60; 20, 35, and 45; and 42, 20, and 38.

TABLE VII

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 13, 27, AND 60

Measures	Bar (Figure 19)	Circle (Figure 13)
Mean	8.15	7.01
Standard deviation	5.92	5.39
Standard error of mean	.21	.19

The percentages 13, 27, and 60 were estimated somewhat more accurately from the circle (Figure 13) than from the bar (Figure 19). The mean error for the bar was 8.15 and for the circle 7.01. The observed difference between the means, which was 1.14, was 4.1 times the standard error of the difference, which was .28.

TABLE VIII

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 20, 35, AND 45

Measures	Bar (Figure 9)	Circle (Figure 26)
Mean	8.80	4.71
Standard deviation	8.88	5.60
Standard error of mean	.31	.20

The circle of Figure 26 was judged more accurately than the bar of Figure 9. The mean error for the bar was 8.80 and for the circle was 4.71. The observed difference (4.09) was 11.1 times the standard error of the difference between the means (.37).

TABLE IX

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 42, 20, AND 38

Measures	Bar (Figure 23)	Circle (Figure 10)
Mean	8.23	7.85
Standard deviation	4.92	5.18
Standard error of mean	.17	.18

Very little difference was shown in the accuracy of judgment of the percentages 42, 20, and 38 when based on the bar of Figure 23 or on the

circle of Figure 10. The mean error in estimating the bar was 8.23 and in estimating the circle 7.85. The observed difference of .38 was but 1.5 times the standard error of the difference between the means, which was .25.

FOUR-PART DIAGRAMS

Three pairs of four-part diagrams were employed, showing the percentages 3, 16, 21, and 60; 10, 19, 28, and 43; and 15, 20, 25, and 40.

TABLE X

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 3, 16, 21, AND 60

Measures	Bar (Figure 18)	Circle (Figure 25)
Mean	7.76	6.86
Standard deviation	6.70	5.53
Standard error of mean	.24	.20

For the percentages 3, 16, 21, and 60 the estimates were more accurate when based upon the circle. The mean error for the bar of Figure 18 was 7.76 and for the circle of Figure 25 was 6.86. The difference between the means (.90) was 2.9 times the standard error of the difference, which was .31.

TABLE XI

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 10, 19, 28, AND 43

Measures	Bar (Figure 27)	Circle (Figure 16)
Mean	8.26	8.03
Standard deviation	4.94	5.01
Standard error of mean	.17	.18

No significant difference was observed between the bar and circle for the percentages 10, 19, 28, and 43. The mean error resulting from judging the bar was 8.26, while that for the circle was 8.03. The difference (.23) was .9 times the standard error of the difference between the means (.25).

The percentages 15, 20, 25, and 40 were shown by the bar of Figure 11 and the circle of Figure 20. In this case the circle was estimated more accurately, the mean error being but 6.30 as compared with 8.24

TABLE XII

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 15, 20, 25, AND 40

Measures	Bar (Figure 11)	Circle (Figure 20)
Mean	8.42	6.30
Standard deviation	7.91	7.32
Standard error of mean	.28	.25

for the bar. The observed difference, of 2.12, was 5.6 times the standard error of the difference, which was .38.

FIVE-PART DIAGRAMS

TABLE XIII

MEASURES OF ERRORS RESULTING FROM ESTIMATES OF BAR AND CIRCLE SHOWING PERCENTAGES 5, 12, 18, 30, AND 35

Measures	Bar (Figure 24)	Circle (Figure 22)
Mean	9.17	9.62
Standard deviation	6.32	6.16
Standard error of mean	.23	.22

Little difference was apparent in the accuracy of estimates based upon the two five-part diagrams. The mean error for the bar of Figure 24 was 9.17 and for the circle of Figure 22 was 9.62. The difference (.45) was but 1.4 times the standard error of the difference (.32).

TABLE XIV

RECAPITULATION OF MEAN ERRORS FOR BARS AND CIRCLES

Diagrams	Mean error for bar	Mean error for circle	Difference÷ σ difference	Advantage in favor of:	Significant?
Two-part diagrams:					
12, 88 (Figures 12 and 2)	4.20	5.25	4.2	Bar	Yes
25, 75 (Figures 4 and 6)	5.34	.58	18.3	Circle	Yes
25, 75 (Figures 4 and 15)	5.34	3.47	6.0	Circle	Yes
29, 71 (Figures 7 and 21)	6.82	3.76	12.2	Circle	Yes
33, 67 (Figures 8 and 3)	4.27	6.08	6.4	Bar	Yes
40, 60 (Figures 1 and 14)	4.43	4.21	.7	Circle	No
50, 50 (Figures 17 and 5)	3.24	.61	13.8	Circle	Yes
Three-part diagrams:					
13, 27, 60 (Figures 19 and 13)	8.15	7.01	4.1	Circle	Yes
20, 35, 45 (Figures 9 and 26)	8.80	4.71	11.1	Circle	Yes
42, 20, 38 (Figures 23 and 10)	8.23	7.85	1.5	Circle	No
Four-part diagrams:					
3, 16, 21, 60 (Figures 18 and 25)	7.76	6.86	2.9	Circle	Yes
10, 19, 28, 43 (Figures 27 and 16)	8.26	8.03	.9	Circle	No
15, 20, 25, 40 (Figures 11 and 20)	8.42	6.30	5.6	Circle	Yes
Five-part diagrams:					
5, 12, 18, 30, 35 (Figures 24 and 22)	9.17	9.62	1.4	Bar	No

CONCLUSIONS

Table XIV presents the mean errors for each of the pairs of diagrams and also indicates whether the differences between the means were or were not significant. It is not possible to make a general statement to the effect that either the bar or the circle was in all cases more desirable for the presentation of component parts. In conclusion a few important points may be noted:—

(1) For presenting a 50–50 relationship the circle was undoubtedly superior to the bar.

(2) For presenting the percentages 25 and 75 the circle was clearly preferable to the bar. This was especially true when the circle was constructed with the 25 per cent section centered at 1:30 o'clock, as in Figure 6, but also obtained when the 25 per cent section was centered at 6 o'clock, as in Figure 15.

(3) In addition to the 50–50 and 25–75 relationships four other pairs of two-part diagrams were included. For these four cases judgment based upon the bar seemed to have been more accurate, since in two instances there was a significant difference in favor of the bar, in one instance a significant difference in favor of the circle, and in one instance a difference that was not significant in favor of the circle.

(4) For the three pairs of diagrams of three parts the circle appeared to have been preferable, there being two cases of a significant difference in favor of the circle, and one case of a difference in favor of the circle, but not significant.

(5) The four-part diagrams, of which there were three, yielded the same results as the three-part diagrams. There were two instances of a significant difference in favor of the circle, and one in favor of the circle, but not significant.

(6) In the case of the single pair of diagrams of five parts no choice could be made, since the difference between the mean errors, while in favor of the bar, was not significant. It was apparently true that the greater the number of sections into which a diagram was divided, the greater the error that a reader was apt to make.

(7) Of a total of 21,789 estimates of diagrams, 466, or slightly more than 2 per cent, failed to total 100 per cent. This was in spite of the fact that the observers were carefully instructed that the parts of each diagram totaled 100 per cent. This error was present to a greater extent in the case of the multiple part diagrams, than in the case of the simpler ones.

THE VALIDITY OF CORRELATION IN TIME SEQUENCES AND A NEW COEFFICIENT OF SIMILARITY

By O. Gressens and E. D. Mouzon, Jr., *University of Illinois*

There has always been some doubt concerning the validity of a strict interpretation of the coefficient of correlation when applied to economic time sequences. The foundations of the doubt, which, with Professor Persons is a conviction against such an interpretation, are based on the mathematical assumptions which underlie the derivation of this coefficient. Granting the assumptions and providing that the mathematical demonstration is sound, any conclusions pertaining to a particular problem in hand must necessarily be accepted. This applies to the coefficient of correlation. If the assumptions underlying the derivation of the coefficient of correlation are granted for the case of economic time sequences, the conclusions that can be drawn from the magnitude of this coefficient must be valid. The question of the applicability of the coefficient of correlation to these sequences of data is, therefore, strictly confined to the assumption underlying the theory of correlation.

The method of variate difference correlation as advanced by "Student" [1] has been successfully attacked by Persons and Yule. The most telling argument against this method is that the influences of time, *per se*, cannot be expressed as a rational integral polynomial in t, and eliminated from the sequence by differencing. Yule has pointed out that if this were done, there would be no residuals.

Yule, in his more recent work on the correlation of time sequences,[2] has shown why nonsense correlations are sometimes obtained with these data. His demonstration, however, is entirely empirical. He does not enter into an examination of the assumptions involved in the theory of correlation.

The assumptions made by Karl Pearson in the derivation of the coefficient of correlation are as follows:

1. The contributory causes which determine the magnitude of the observed data are *independent.*

2. The variations in intensity of these causes are *small as compared with their absolute intensity.*

[1] *Biometrika*, Vol. 10. "Elimination of Spurious Correlation due to Position in Time or Space."

[2] *Journal of the Royal Statistical Association*, March, 1926. "Why We Sometimes Obtain Nonsense Correlation in Time Series."

3. The *variations* in intensity of the contributory causes *follow the normal law of distribution.*

In consideration of the nature of economic sequences, are these assumptions legitimate? This question necessitates an examination of the nature of these data.

The exchange relationships which obtain between economic goods invalidate the first of these assumptions for economic time sequences. The causes which determine the magnitudes of any such sequence of economic data have, through the phenomena of exchanges, been transmitted to become a part of the nexus of causes which affect innumerable other sequences of data. This complex of interdependence created by exchange relationships makes it impossible to postulate independent causes. Concretely, the value of any economic good is not an independent phenomenon, but it is interrelated with the value of every other economic good available on the market.[1] The price of steel, for instance, depends on the prices of all other commodities available in the market. The nexus of causes, therefore, which influence the prices of all other commodities are dependent upon and form a part of the nexus of causes which determines the price of steel. The magnitudes of any economic time sequences are similarly interdependent. The first assumption underlying the theory of correlation, therefore, is invalid for the case of these data. This, in itself, is a sufficiently telling argument against a strict interpretation of correlation in the case of economic time sequences.

The variations in intensity of these causes, as compared with their absolute intensity are not necessarily small, and, in fact, furnish some of the most difficult of all economic problems. Technical improvements in production, sudden changes in demand caused by the introduction of new consumption goods or the increased efficiency in the production of old goods, changes in population characteristics owing to immigration, discovery of new resources, or sudden cessation of old resources, and all of the multitude of changes, swift and unforeseen, which root in the dynamic characteristics of economic development, bring about significant variations in the intensities of the causes that determine the magnitudes of economic data. For example, the disturbances in price which were caused by the issues of greenbacks during the Civil War, the crippling of the bicycle industry in the United States by the introduction of automobiles, and the present plight of agriculture, owing to a combination of violent changes such as the decline of the

[1] The case of a newly introduced good offers a temporary variation from this condition, because, for a short period, the price of this good may be arbitrarily fixed. Even here, however, a relationship is immediately established, for the fixed price is reflected on the values of other goods, if the good can be moved at the arbitrary price. An exchange unavoidably creates a relationship.

hay and fodder crop caused by the introduction of automobiles, tractors, etc., are all instances of large variations of contributory causes. The second assumption involved in the derivation of the coefficient of correlation is, therefore, not tenable in the case of time-ordered sequences.

The third assumption that the variations of the causes affecting the data under consideration distribute themselves according to the normal law of error is not necessarily true. No definite assumptions can be made concerning the size of the variations of the causes. Equal numbers of positive and negative variations of the same magnitude cannot be judiciously postulated. In the case of these data, no valid assumption can be made concerning the likelihood of large or small variations. In consideration of the nature of these data, therefore, the third assumption is entirely arbitrary and cannot be accepted. Since all of the above assumptions are invalid for time-ordered sequences, the theory of correlation cannot logically be applied to these data.

Professor Persons has recognized that the coefficient of correlation, as derived by Professor Pearson, is not applicable. He uses Professor Pearson's product moment coefficient, but derives it empirically, not as a measure of correlation, but as a means for determining maximum lag. This is done by considering the coefficient as a measure of covariation and determining the point of maximum effect of lag as that point where the coefficient is greatest. Professor Irving Fisher has explained that the lag between two sequences is distributable. The maximum ordinate of the curve describing Fisher's distributed lag is his maximum effect of lag, while the greatest value of Persons' coefficient determines the point at which he describes the maximum effect of lag.

An investigation of Professor Fisher's method of distributing lag, however, discloses that the essence of his procedure is trial and error. The object of his method is to obtain maximum correlation. His distribution curve is chosen so that one sequence will resemble the other as nearly as possible. This comes perilously close to a manipulation of data.

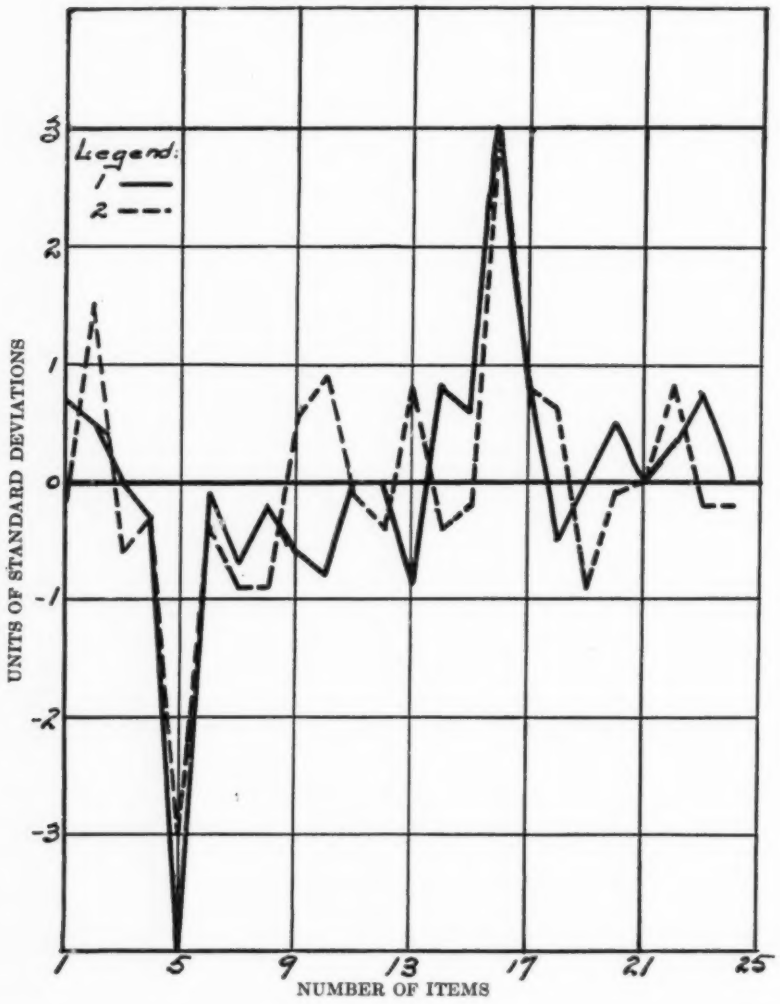
Neglecting for a moment Professor Fisher's criticism of the product moment coefficient as a measure of lag, there is another serious objection to this means of determining similarity between sequences. The coefficient cannot be relied upon to test covariation, and to determine the point at which covariation is greatest.

In testing the two sequences for similarity by shifting them, it may occur that large fluctuations in the same direction are paired. The magnitude of the product of these large items unduly weights the

product sum, so that a higher degree of similarity is indicated than is warranted from the remaining fluctuations of the sequences. In fact, a higher degree of similarity may be indicated in a pair of sequences in which only a few large items are concurrent than in the case of a pair

CHART I

CYCLE FLUCTUATIONS OF SEQUENCES 1 AND 2 (TABLE I)

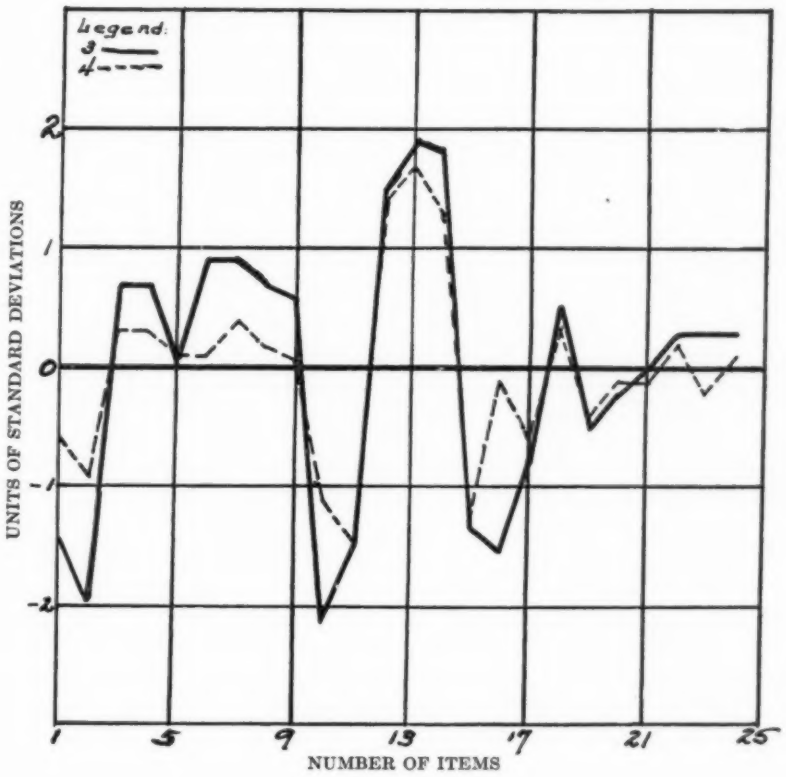


of sequences almost continually moving in the same direction. Take, for example, the two pairs of sequences illustrated in Chart I and Chart II. These sequences are already expressed in units of standard devia-

tions. It is obvious from an inspection of the charts that the sequences of Chart II are much more similar than those of Chart I. The general tendency of the sequences of Chart II is in the same direction throughout the period, while no regularity of similar fluctuations is observable in the sequences of Chart I. If the product moment coefficient were a reliable measure of similarity, the magnitude of this coefficient would be greater for the sequences of Chart II than for those of Chart I.

CHART II

CYCLE FLUCTUATIONS OF SEQUENCES 3 AND 4 (SEE TABLE I)



As a matter of fact the obverse is the case. For the sequences of Chart I, $r = .86$, while for those of Chart II, $r = .75$.

Although in the majority of cases a coefficient indicating degree of similarity is not necessary, and when used is applied to cycle data arbitrarily determined and, therefore, quantitatively inexact, a more simple coefficient, not subject to the above criticism, can readily be

obtained.[1] It may be called S, the coefficient of similarity. It is calculated as follows:

1. The "cycle" movements of the sequences to be compared are isolated by the usual methods and expressed in units of their respective standard deviations.[2]

2. The differences between corresponding "cycle" items of these sequences are then obtained.

3. The average deviation of these differences is calculated.

4. This average deviation is divided by the sum of the average deviations of the "cycle" items of the sequences which are being compared. This serves to make comparable the coefficients of several such comparisons.

Symbolically, this can be expressed as follows:

Let a_1, a_2, $- - - a_n$, and b_1, b_2, $- - - b_n$ represent the "cycle" fluctuations, expressed in terms of standard deviations, of the two respective sequences. The coefficient then is

$$S=\frac{\dfrac{|a_1-b_1|+|a_2-b_2|+ - - - - - - |a_n-b_n|}{n}}{\dfrac{|a_1|+ - - - |a_n|}{n}-\dfrac{|b_1| - - - - - |b_n|}{n}}$$

or $$\frac{A.D.(a_i-b_i)}{A.D.(a_i)+A.D.\ (b_i)}.$$

The upper limit of this coefficient is unity. This can readily be seen from the following considerations:

$$S=\frac{\dfrac{\Sigma|a_i-b_i|}{n}}{\dfrac{\Sigma|a_i|}{n}+\dfrac{\Sigma|b_i|}{n}}=\frac{\Sigma|a_i-b_i|}{\Sigma|a_i|+\Sigma|b_i|}=\frac{\Sigma|a_i-b_i|}{\Sigma(|a_i|+|b_i|)}.$$

The a_i's and b_i's, being real numbers, the maximum value of the numerator is given by the denominator. S, therefore, cannot exceed unity.

This coefficient, as Professor Persons', is, of course, empirical. It has several distinct advantages, however, as compared with the product moment coefficient.

1. It is completely divorced from any idea of correlation, and thus

[1] It was originally suggested by Mr. Mouzon that a coefficient based on "cycle" differences might be found useful. This coefficient was subsequently worked out in detail and applied to a number of cases by the authors.

[2] Units of average deviations may be used.

causes no confusion with this theory. The adoption of the product moment coefficient as a measure of similarity has resulted, in many quarters, in the mistaken conclusion that the theory of correlation is applicable to economic time sequences.

2. Unusual fluctuations in the same direction of paired observations do not unduly influence the magnitude of the coefficient, and thus indicate similarity between two sequences, which in fact is due only to a few sporadic fluctuations. If the "cycle" movements of two time-ordered sequences are exactly alike, this coefficient is zero, for the differences of the "cycle" curves, in units of standard deviations, are all zero. The coefficient increases in magnitude as the sequences that are being compared become more dissimilar, the upper limit being unity.

3. The coefficient is easier to calculate than is the product moment coefficient, and it is of a more simple nature. After all, it is only the average of the absolute values of the differences between the paired "cycle" items of compared sequences. The coefficient is made comparable as between several such pairs of data by dividing this average by the sum of the average deviations of the two sequences.

Referring back to the examples of Chart I and Chart II, the coefficient S has a magnitude of .48 and .27 respectively. It is immediately evident that the coefficient is a much more reliable measure of similarity than is the product moment coefficient. Thus the coefficient S for the example in Chart II is slightly more than half as large as that of the example in Chart I, indicating a much higher degree of similarity. It will be remembered that the product moment coefficient had a magnitude of .86 for Chart I and .75 for Chart II. Obviously S is a much more reliable measure of similarity.

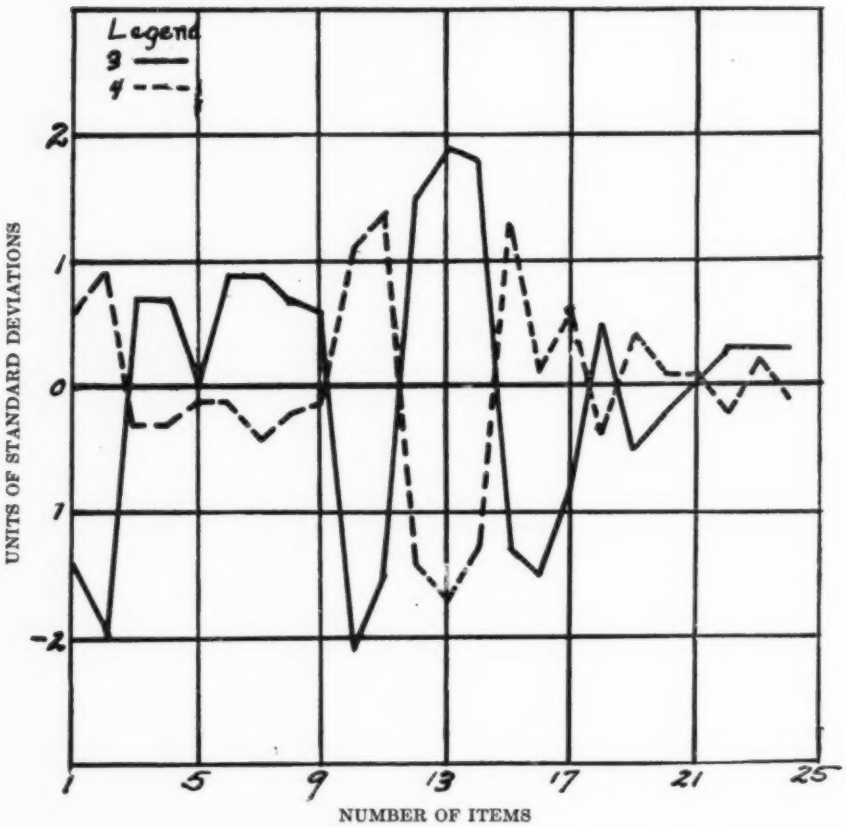
The lag obtained by the use of the coefficient of similarity may be different from the lag which would be indicated by the product moment coefficient. The coefficient S has been applied to a number of paired sequences, and it has been found that the lag in many cases is the same. The main features of the coefficient, however, are that it is more reliable, particularly in unusual cases, and that it is simpler to calculate.

As was stated above, the higher values of this coefficient indicate increasing dissimilarity between the compared sequences, the greatest dissimilarity being indicated when the coefficient takes on the value unity. A case in which the coefficient closely approximates unity is illustrated by the sequences presented with Chart III. In this case one of the sequences of Chart II has been inverted. The coefficient, S, here has a value of .98, indicating a high degree of dissimilarity. This example also illustrates another property of the coefficient. As it approaches its upper limit, unity, the condition obtaining between

the compared sequences would cause the value of the coefficient of correlation to be negative. Continuous fluctuations in opposite direction, therefore, constitute the condition under which the value of the coefficient S is unity. A condition comparable to the case where the coefficient of correlation is zero obtains when S has the same value for a direct and inverse comparison between two sequences.

CHART III.

CYCLE FLUCTUATIONS OF SEQUENCE 3 AND INVERTED SEQUENCE 4



The claim advanced for this coefficient is that it measures the similarity between any two sequences better than does the product moment coefficient and is, therefore, a more reliable indication of the maximum effects of lag. Just as in the case of Persons' product moment coefficient, nothing can be said concerning its probable error. As a matter of fact there can be no probable error. The notion of probable error depends on the possession of a number of measurements for each magni-

tude.[1] In the case of these data, however, there is only one measurement for each observation. There is, therefore, no "most probable value" for any observation. The cardinal consideration, therefore, is the *similarity of the variations of the successive magnitudes* of the sequences, and not the numerical instances of a particular magnitude, from which a "most probable value" for that magnitude can be determined.

The isolation of the "cycle" from a sequence of time-ordered data cannot be founded on a separation of the causes which determined the

TABLE I

CYCLE DATA*

Sequences illustrated by Chart I				Sequences illustrated by Chart II				Chart III
1	2	Products	Differences	3	4	Products	Differences	Algebraic sum of 3 and 4
.7	− .4	− .28	1.1	− 1.4	− .6	.84	− .8	− 2.0
.5	1.5	.75	− 1.0	− 1.8	− .9	1.62	− .9	− 2.7
0	− .6	0	.6	.7	.3	.21	.4	1.0
− .3	− .3	.09	0	.7	.3	.21	.4	1.0
−4.0	− 3.0	12.00	− 1.0	.0	.1	0	− .1	.1
− .1	− .4	.04	.3	.9	.1	.09	.8	1.0
− .7	− .9	.63	.2	.9	.4	.36	.5	1.3
− .2	− .9	.18	.7	.7	.2	.14	.5	.9
− .6	.5	− .30	− 1.1	.6	.1	.06	.5	.7
− .8	.9	− .72	− 1.7	− 2.1	− 1.1	2.31	− 1.0	− 3.2
0	− .1	0	+ .1	− 1.5	− 1.4	2.10	− .1	− 2.9
0	− .4	0	+ .4	1.5	1.4	2.10	.1	2.9
− .9	.8	− .72	− 1.7	1.9	1.7	3.23	.2	3.6
.8	− .4	− .32	1.2	1.8	1.3	2.34	.5	3.1
.6	− .2	− .12	.8	− 1.3	− 1.3	1.69	0	− 2.6
3.0	3.0	9.00	0	− 1.5	− .1	− .15	− 1.6	− 1.4
.9	.8	.72	.1	− .8	− .6	.48	− .2	− 1.6
− .5	.6	− .30	− 1.1	.5	.4	.20	.1	.9
0	− .9	0	+ .9	− .5	− .4	.20	− .1	− .9
.5	− .1	− .05	.6	− .2	− .1	.02	− .1	− .3
0	0	0	.0	0	− .1	0	.1	− .1
.3	.8	.24	− .5	.3	.2	.06	.1	.5
.7	− .2	− .14	.9	.3	− .2	− .06	.5	.1
0	− .2	0	+ .2	.3	.1	.03	.2	.4
16.1**	17.9**	20.70	16.2**	22.2**	13.4**	18.08	9.8**	35.2**
	$r = .86$ $S = .48$				$r = .75$ $S = .27$			$r = -.75$ $S = .98$

* In units of Standard Deviations.
** Absolute sums.

magnitudes of the observations. It is, on the other hand, a methodological device, used to compare more readily the relatively short-time fluctuations which often occur in these data. It cannot, therefore, be said that the "cycle" of any particular sequence is an exact quantitative measurement. In a comparison of two sequences of such isolated "cycle" data there is no assurance that the same, or similar causes have determined the magnitudes of the "cyclical" fluctuations. The

[1] A magnitude is used here in the sense of a measurable characteristic.

fundamental assumptions underlying the theory of correlation are not applicable to economic time sequences, as has been discussed above. A comparison of such sequences resolves itself, therefore, into a question of the similarity of the short-time oscillations over a period of time, rather than into a question of the correlation of such data. The element of arbitrariness which enters into the isolation of the "cycles" furnishes an additional argument against the application of correlation to these data, aside from the objections owing to the nature of the data themselves. This defect of the data, of course, is an equally valid objection to *any* comparison between such sequences.

Since the data of these sequences are quantitatively inexact in the form in which they lend themselves to the calculation of descriptive coefficients, it is logical to conclude that the numerical significance of such coefficients is dependent, in part at least, upon the nature of the sequences in hand, and can be considered only as supplementary evidence. For such purposes the coefficient of similarity developed above is superior to the product moment coefficient as derived by Professor Persons. Its calculation is more simple, it is more reliable since it is not influenced in nearly the same degree as the product moment coefficient [1] by large, sporadically paired items, and it is completely divorced from the theory of correlation, which cannot be judiciously applied to economic time sequences.

[1] If the coefficient were not influenced at all by the extent of fluctuations, it could be strictly defined as a coefficient of covariation.

NOTES

ADJUSTMENTS FOR THE INFLUENCE OF EASTER IN DEPARTMENT STORES SALES

By Aryness Joy and Woodlief Thomas, *Division of Research and Statistics, Federal Reserve Board*

Sales of department stores, as measured by the indexes of the Federal Reserve Board, show marked seasonal fluctuations, which from year to year are fairly regular in magnitude. It is possible to secure for most of the months satisfactory measures of these seasonal variations and to use these measures to compute index numbers adjusted for seasonal variations. Fluctuations in March and April sales, however, vary from year to year in direct relation to the date of Easter. The fact that Easter, which is second only to Christmas in increasing the volume of retail trade, may occur as early as March 22 or as late as April 25 makes adjustment by means of a single set of adjustment factors unsatisfactory. When Easter comes as early as March 25, retail sales are much greater in March than they would otherwise be, and if it comes later than the middle of April, April sales are large and those of March proportionately small.

For the sake of accurate comparisons of sales in the spring months, a sliding scale of adjustment factors, varying with the date of Easter, has been devised for removing the seasonal influence of Easter from the Federal Reserve Board's figures. Five different sets of these seasonal factors were derived for the months of March and April and used with constant seasonal adjustment factors for the other ten months, so that the total was always 1,200. The Easter adjustment factors for department store sales for the United States are shown in the following table. The figures in parentheses indicate the sliding scale of differences between the adjustment factors used for March and April when Easter falls in the respective periods shown and the *average* adjustment factors for those two months; *i. e.*, those which are used when Easter comes in the middle period, April 5 to April 8.

This scale of adjustment factors was derived in the following manner:

(1) One set of twelve monthly seasonal adjustment factors was obtained by the ratio-to-moving-average method and adjusted to total

TABLE I

EASTER SEASONAL ADJUSTMENT FACTORS FOR DEPARTMENT STORE SALES

Date of Easter	Adjustment Factors	
	March	April
Before March 31	96 (+3)	98 (−3)
April 1–April 4	95 (+2)	99 (−2)
April 5–April 8 (average)	93 —	101 —
April 9–April 14	91 (−2)	103 (+2)
After April 15	90 (−3)	104 (+3)

1,200.[1] The preliminary "adjusted" curve resulting from the application of these factors to the unadjusted relatives contained in every year a March or April peak, directly attributable to the Easter sales and varying in intensity according to the date of Easter. This is shown on the chart by the broken-line curves representing the index adjusted by means of constant seasonal factors.

(2) This curve was then studied and the differences between the relatives for March and April in each year were calculated and arranged in order according to the date of Easter. For example, in 1924, when Easter was on April 20, the preliminary "adjusted" relative of department store sales for March was approximately 96 and for April 102, a difference of eight points.

(3) From a study of these differences, the amount by which the average adjustment factors (see paragraph 1 above) must be raised or lowered approximately to smooth the preliminary "adjusted" curve was determined. Within the period of the index, for example, Easter has occurred after the 15th of April in four years—1919, 1922, 1924, and 1927. To adjust this group of March and April relatives, it was necessary to lower the average March adjustment factor (93) to 90, and raise the average April factor (101) by the same amount to 104. In a similar experimental fashion adjustment factors for the other periods shown in Table I were derived.

The length of the periods chosen and changes in the sliding scale are not uniform because experience indicates that the distribution of sales between March and April caused by the irregularity of Easter does not vary exactly in proportion to the number of days between the actual dates of Easter in different years. For example, if Easter falls in March, all of the Easter trading will presumably be in that month, hence all

[1] This method was explained in descriptions of Federal Reserve Board production indexes published in the Federal Reserve Bulletin for December, 1922, and for March, 1927.

A full description of the computation of these seasonal factors for department store sales will be presented, together with revised index numbers of department store sales, in a forthcoming issue of the *Federal Reserve Bulletin*.

dates in March are grouped in one period; or if in the first four days of April, most of the Easter sales will be in March and therefore April 1 to April 4 was considered as one period; if between April 5 and April 8, selected as the average period, there are sufficient days in April before Easter to permit some Easter trading in that month. If Easter is later than April 8, a considerably larger portion of the sales are made in April, but the difference of a few days causes a much smaller relative variation in sales, therefore, the period is made longer, extending to April 14. If Easter comes at any time after April 14, the exact date

has practically no significance in respect to the distribution of sales between March and April. Therefore, a uniform scale of difference was applied to this period.

(4) The unadjusted relatives for March and April were then divided by appropriate adjustment factors to obtain the adjusted index.

The dates of Easter for the years included in the index and the scale of differences to be applied to the average adjustment factors (those used for the period April 5–April 8) in order to adjust for Easter variations are shown in Table II, and the newly adjusted indexes are shown by the heavy lines on the chart.

TABLE II

Year	Date of Easter	Difference from average adjustment factor	
		March	April
1919	April 20	−3	+3
1920	April 4	+2	−2
1921	March 27	+3	−3
1922	April 16	−3	+3
1923	April 1	+2	−2
1924	April 20	−3	+3
1925	April 12	−2	+2
1926	April 4	+2	−2
1927	April 17	−3	+3
1928	April 8	Average	Average

This sliding scale of adjustment factors is reasonably effective in the elimination of Easter variations in the indexes of department store sales for the years 1919–1927. It is purely experimental and empirical, for it is based upon the experience of a relatively short time, and its best test will be whether it removes the seasonal influence of Easter in the next few years. The exact differences derived for use in this particular case, of course, are not applicable to other series. For apparel stores, for example, larger differences from the average are necessary to adjust for Easter variations. The method by which these adjustment factors for department store sales were derived suggests an empirical procedure which may be applicable to other series in which seasonal shifts are caused by a similar influence.

THE DOOLITTLE METHOD FOR SOLVING MULTIPLE CORRELATION EQUATIONS VERSUS THE KELLEY-SALISBURY "ITERATION" METHOD

By H. R. Tolley and Mordecai Ezekiel

In presenting a new "iteration" method for solving normal equations by the use of conversion formulae[1] Kelley and Salisbury make the statement "that it cuts down the labor of a 16 variable problem from that of a solution by determinants by 95 per cent is a conservative estimate." From this statement it would seem that they are unfamiliar with the many methods for solving normal equations which are far superior to the use of determinants. Gauss himself developed a direct process of elimination which was much shorter than solution by determinants.[2] Methods of elimination by successive trials and approximations were also suggested by Gauss, and as early as 1855 the United States Coast Survey was employing various methods of this kind.[3] Later, however, M. H. Doolittle of the United States Coast and Geodetic Survey made various improvements on the direct method of solution developed by Gauss, and by 1878 this direct method had apparently entirely replaced the earlier approximation methods.[4] The Doolittle method has been subsequently described in standard texts on least squares,[5] and is in customary employment in the computation of geodetic corrections.

The advantages of the Doolittle method for multiple correlation work were pointed out by the present authors in 1923,[6] and it has subsequently been incorporated in several statistical texts,[7] as well as

[1] Truman L. Kelley, and Frank S. Salisbury, "An Iteration Method for Determining Multiple Correlation Constants," this Journal, September, 1926.

[2] J. F. Encke, "Über die Methode der kleinsten Quadrate," *Astronomische Jahrbuch*, Berlin, pp. 267–272, 1835; p. 265, 1836.

[3] Charles A. Schott, "Solution of Normal Equations by Indirect Elimination," *Report of the Superintendent of the United States Coast Survey*, 1855, Appendix No. 40, pp. 255–264.

[4] M. H. Dooilttle, "Adjustment of the Primary Triangulation between Kent Island and Atlanta Base Lines" (Paper No. 3, "Method Employed in the Solution of Normal Equations and the Adjustment of a Triangulation"), *Report of the Superintendent, Coast and Geodetic Survey*, 1878, pp. 115–120.

[5] Thomas W. Wright and John Fillmore Hayford, *Adjustment of Observations by Method of Least Squares with Applications to Geodetic Work*, 1906, pp. 114–121. For other references, see Oscar S. Adams, *Special Publication 28*, United States Coast and Geodetic Survey.

[6] H. R. Tolley and M. J. B. Ezekiel, "A Method of Handling Multiple Correlation Problems," this Journal, December, 1923, p. 995.

[7] Frederick C. Mills, *Statistical Methods Applied to Economics and Business*, 1924, pp. 491–498, 577–581.

Bradford B. Smith, "The Use of Punched Card Tabulating Equipment in Multiple Correlation Problems." Bureau of Agricultural Economics, mimeographed report, 1923.

H. A. Wallace and George W. Snedecor, "Correlation and Machine Calculation," *Iowa State College, Bulletin 35*, 1925.

been taught and used in many institutions. Since, however, so eminent a statistician as Dr. Kelley has apparently not become acquainted with it, it would seem worth while taking space in this JOURNAL to present this technique, which gives exact results to five decimal places and an automatic check on the accuracy of all the arithmetic with approximately one-half the work required by the iteration method to get results accurate only to the second decimal place.

The subsequent arithmetic example shows for the same set of values used in the Kelley-Salisbury article every calculation required to obtain the net regression coefficients and the coefficient of multiple correlation by the Doolittle method. In comparing this example with the tables shown in the Kelley-Salisbury paper (pages 283 and 284) it should be mentioned that many of the auxiliary computations required by their method and covered in the text are not shown in their tables. The volume of work required by the two methods may be judged from the following comparative statement, based on an actual count of the two procedures:

NUMBER OF COMPUTATIONS REQUIRED TO SOLVE NORMAL EQUATIONS FOR THE COEFFICIENTS OF NET REGRESSION AND MULTIPLE CORRELATION FOR A SIX-VARIABLE PROBLEM

Methods	Multiplications	Divisions	Additions	
			Totals made	Items added
Doolittle	84	21	34	129
Kelley-Salisbury *	163	12	84	207

* Approximations carried only so far as shown in Table II of the paper by Kelley and Salisbury, p. 283.

The relative accuracy of the results obtained by the two methods may be judged from the following statement.

VALUES OBTAINED BY DIFFERENT METHODS

Constant	Kelley-Salisbury method*	Doolittle method	Correct values**
$b_{01.2345}$	0.197	0.194124	0.19412341
$b_{02.1345}$	.321	.323927	.32392693
$b_{03.1245}$	.023	.027484	.02748474
$b_{04.1235}$	.441	.436940	.43693997
$b_{05.1234}$	.130	.134664	.13466545
$R_{0.12345}$	.79006	.790090	.79009053

* Approximations carried as far as in previous table.
** As given by Kelley and Salisbury, p. 286.

It is evident that the iteration method gave regression coefficients still in error by 1 per cent to 16 per cent of their correct values when the iterations were carried to a stage requiring twice the volume of compu-

TABLE I

DOOLITTLE SOLUTION OF NORMAL EQUATIONS[1]

Line Designation	b_2	b_3	b_4	b_5	b_6	L	Σ	
				Column Designation				
				Equations to be Solved				
Eq. I	100.00	23.32	19.86	25.69	10.64	40.17	219.68	
Eq. II	(23.32)	100.00	17.47	45.20	21.39	60.03	267.41	
Eq. III	(19.86)	(17.47)	100.00	26.28	0.33	23.79	187.73	
Eq. IV	(25.69)	(45.20)	(26.28)	100.00	29.89	68.07	295.13	
Eq. V	(10.64)	(21.39)	(0.33)	(29.89)	100.00	35.53	197.78	
				Front Solution				
I′	100.0000	23.3200	19.8600	25.6900	10.6400	40.1700	219.6800	
I-a	−1.000000	−.233200	−.198600	−.256900	−.106400	−.401700	−2.196800	
II		100.0000	17.4700	45.2000	21.3900	60.0300	267.4100	
II-1		−5.4382	−4.6314	−5.9909	−2.4812	−9.3676	−51.2294	
II′		94.5618	12.8386	39.2091	18.9088	50.6624	216.1806	
II-a		−1.000000	−.138769	−.414640	−.199962	−.535759	−2.286130	
III			100.0000	26.2800	0.3300	23.7900	187.7300	
III-1			−3.9442	−5.1020	−2.1131	−7.9778	−43.6284	
III-2			−1.7431	−5.3234	−2.5672	−6.8784	−29.3506	
III′			94.3127	15.8546	−4.3503	8.9338	114.7510	
III-a			−1.000000	−.168107	+.046126	−.094725	−1.216706	
IV				100.0000	29.8900	68.0700	295.1300	
IV-1				−6.5998	−2.7334	−10.3197	−56.4358	
IV-2				−16.2577	−7.8403	−21.0067	−89.6371	
IV-3				−2.6653	+0.7313	−1.5018	−19.2904	
IV′				74.4772	20.0476	35.2418	129.7667	
IV-a				−1.000000	−.269178	−.473189	−1.742367	
V					100.0000	35.5300	197.7800	
V-1					−1.1321	−4.2741	−23.3740	
V-2					−3.7810	−10.1306	−43.2279	
V-3					−0.2007	+0.4121	+5.2930	
V-4					−5.3964	−9.4863	−34.9303	
V′					89.4898	12.0511	101.5408	
V-a					−1.00000	−.134664	−1.134664	

	Back Solution					Eq. V	Check	Eq L	Comp. of R^2
L	+.401700	+.535759	+.094725	+.473189	+.134664				
b_6	−.014328	−.026928	+.006212	−.036249	+.134664	100.00	13.4664	35.53	4.7846
b_5	−.112250	−.181173	−.073453	+.436940		29.89	13.0601	68.07	29.7425
b_4	−.005458	−.003731	+.027484			.033	.0091	23.79	.6538
b_3	−.075540	+.323927				21.39	6.9288	60.03	19.4453
b_2	+.194124					10.64	2.0655	40.17	7.7980
					Eq. V=35.53		35.5299		62.4242

$$R^2=\frac{62.4242}{\sigma_1^2}=\frac{62.4242}{100.00} \qquad R=\sqrt{.624242}=.790090$$

[1] The detailed explanation of this form of solution is given in F. C. Mills, *Statistical Methods*, pp. 578–581.

tations required by an exact solution by the Doolittle method. Since the Doolittle method does give an exact solution, the accuracy of the results depends solely upon the number of decimal points carried in the work, and no greater number of computations need be performed to obtain results accurate to 10 places, if desired. The example shown, with results correct to 5 places, was calculated in 50 minutes by Miss Helen L. Lee of the Bureau of Agricultural Economics, using an electric Monroe. Since astronomical accuracy of regression coefficients is really not necessary anyway, the results are more accurate than is ordinarily required; in usual work the Doolittle solution is carried to two less places than is shown in the accompanying arithmetic example. Carried that far it took Miss Lee 40 minutes to work through this same solution, giving answers correct in this test to the fourth decimal place. Answers correct to 2 decimal places, similar to those obtained by the iteration method, can be computed by the Doolittle method using only a 20-inch slide-rule.

Besides requiring fewer computations, the Doolittle method has a great advantage in that it follows a regular fixed routine, requiring no judgment on the part of the computer. In addition, as is shown in the accompanying arithmetic example, it provides an automatic check on each stage of the work, which shows up errors almost as soon as made and insures accuracy throughout. As a consequence clerks having no mathematical training can readily learn the routine of the method and carry through the solution of the normal equations quickly and accurately without technical supervision.

It is barely possible that for problems of 15 variables or more the iteration method may possess advantages over the Doolittle method which do not appear in this smaller problem. From the lack of accuracy in this small problem, however, it would seem improbable that such would be the case. It would certainly be desirable to lighten the arithmetic of multiple correlation problems, which is heavy enough even with the Doolittle method; but until further evidence is forthcoming it would seem that the iteration method has not done so.

THE DETERMINATION OF THE MEAN DEVIATION FOR GRADED DATA

By Margaret H. Hogg

Although the determination for graded data of every measure of deviation requires some assumption whose implications must be analyzed, so many textbooks overlook this necessity in dealing with the mean deviation that comment upon its case seems justifiable.

G. U. Yule in his *Introduction to the Theory of Statistics* offers an example of a mean deviation for graded data calculated on the assumption that all cases are concentrated at the middle points of their respective grades, but earlier in the book he has postulated that the grades must be narrow. Moreover, in dealing with this example he points out that the more justifiable assumption of uniform distribution of cases within their respective grades involves an additive correction for the deviations for that grade in which the median or mean lies, although it leaves aggregate deviations for all other grades unchanged. In his example the correction is insignificant, as he points out in an exercise set to the reader, and this seems to have had the unfortunate result of encouraging some other writers to leave it unnoticed; although, as it only involves processes already familiar, it provides without any new kind of complication a very useful test of the student's assimilation of methods by the readiness with which it is appreciated. Furthermore, some writers have not only not repeated the warning to avoid wide grading, but also have not themselves avoided it in their choice of examples. In fact, out of twenty-five examples found in a random collection of textbooks (after rejection of one more showing no tendency toward a central type), seventeen have spurious accuracy in their results which is due to disregard of this, seven being correct only to one significant figure out of three (a few having been first modified because incorrect even with the assumption of concentration). One consequence of neglecting the effect of the assumption of concentration is to make the mean deviation from the true median appear greater than that from the center of the median grade, because under that assumption the median is of course at the center of the grade. Efforts to avoid this unsatisfactory result offer the most probable explanation of soms curious aberrations from logic to be found in occasional examples.

The correct aggregate deviation for a central grade on the assumption of uniform distribution is worked out in the *Handbook of Mathematical Statistics* edited by H. L. Rietz (pages 29, 30), but it may be con-

venient to quote here the result in the alternative form of a correction to be added to the result reached under the first assumption (of concentration). This is $\frac{n}{N}\frac{(\frac{h}{2}-d)^2}{h}$, where $\frac{n}{N}$ equals the proportion of observations found in the central grade, h width of grade, d distance of median or mean from center of grade.

Even Rietz does not give any warning as to the effect of this second supposition of uniform grades, nor does Yule. A. L. Bowley in his *Elements of Statistics* points out that the exaggerating effect of this

TABLE I

SUBTRAHEND TO CORRECT FOR EFFECT OF SUPPOSED UNIFORMITY OF DISTRIBUTION WITHIN GRADES, CALCULATED FROM THE NORMAL CURVE

$\frac{h}{\sigma}$	$\frac{\eta'}{\sigma}$	Per cent Subtrahend	$\frac{\eta'}{h}$	$\frac{\Sigma_1^4 \eta}{N}$
.1	.7986	.09	7.986	.159
.2	.8000	.26	4.000	.311
.25	.8020	.51	3.280	.383
.3	.8039	.75	2.680	.451
.4	.8087	1.3	2.022	.576
.5	.8145	2.0	1.629	.683
.6	.8220	2.9	1.370	.770
.75	.8355	4.5	1.113	.866
.8	.8410	5.1	1.051	.890
.9	.8527	6.4	.947	.928
1.0	.8656	7.8	.866	.954
1.2	.8964	11.0	.747	.984
1.25	.9047	11.8	.724	.988
1.5	.9543	16.4	.636	.997
1.6	9776	18.4	.611	.999
1.8	1.0300	22.5	.572	.9996
2.0	1.0912	26.9	.546	1.0

h = width of grade.
η' = mean deviation tabulated from graded data.
$\frac{\Sigma_1^4 \eta}{N}$ = proportion of observations found in 4 central grades.

supposition is such as to make the mean deviation unsuitable for any but narrow grades, but pursues the point no farther. Now if mean deviations are to be computed, as they are being computed, for data with all varieties of width of grade, it seems desirable that some kind of estimate should be available of the degree of error to be expected, in order that we may avoid spurious accuracy from this second cause.

It is obvious that in the ordinary frequency group where observations cluster round a central type, the effect of supposing uniformity of distribution within grades must be to enlarge the mean deviation, henceforward called η, following Bowley, just as it is to enlarge the standard deviation σ, the error of course increasing with the width of the grade. To provide some reasonable surmise as to the amount of

exaggeration to be expected, the effect has been calculated of assuming uniform distribution within grades in the case of the Normal Curve of Error. The percentage to be subtracted from the resulting mean deviation η' has been found for grades of different width, and with it the ratio of the uncorrected η' to the width of the grade, $\frac{\eta'}{h}$, and also the proportion of the group to be found in the four grades to whose center the median or mean is nearest, $\frac{\Sigma_1^4 n}{N}$.

Now if the prima facie likelihood is that the distribution within

TABLE II

	Percentage Subtrahend deduced from		Proportion of significant places remaining correct on this assumption
	$\frac{\eta'}{h}$	$\frac{\Sigma_1^4 \eta}{N}$	
Example 1	8.4	9.2	1/3
2	4.2	3.5	1/3
3	3.6	3.9	2/3
4	3.1	2.7	1/3
5	2.7	2.5	2/3
6	2.4	2.7	1/3
7	2.3	2.2	2/4
8	2.0	2.1	1/3
9	1.85	1.9	2/4
10	1.7	1.7	1/3
11	1.6	1.9	1/3
12	1.5	1.9	1/4
13	1.5	1.8	1/4
14	1.4	1.45	1/3
15	1.3	1.3	2/4
16	1.3	1.1	2/3
17	1.3	1.3	1/3
18	1.2	1.35	1/2
19	1.05	1.2	2/5
20	1.05	1.2	2/3
21	.9	1.9	2/4
22	.8	.7	1/3
23	.75	.8	2/3
24	.7	.7	2/4
25	.6	.7	1/3
26	.55	.7	2/3
27	.4	.55	2/3
28	.3	.9	1 or 2/3

Example 2 is appreciably skew; Example 21 very skew;
Example 28 a small and peculiar group.

grades resembles that of the Normal Curve more nearly than it does uniformity, as will usually be the case, it seems probable that by subtracting the appropriate correction applicable to the Normal Curve we shall arrive at a figure for η which has a better chance of accuracy than that calculated on the supposition of uniformity, though for safety the wisest course might be to quote η only to those significant figures which the proposed correction leaves unaltered.

It is next proposed that, since σ has rarely been evaluated in the example under consideration, the proportion borne by the calculated η' to the width of grade should be used to indicate the size of the relative correction necessary. Alternatively, to obtain a rough idea of that relative correction before embarking on the process of computation of η, we may try the effect of using the proportion of the group found in the four central grades to give indication, and proceed or abstain in consequence.

The effect of grading on the mean deviation from the median for skew curves has been tested (approximately, taking $\frac{\kappa\sigma}{6}$ to be the distance between median and average) in the case of the Second Approximation to the Generalized Curve of Error by comparing the mean deviation from the true median in the continuous curve with that of the graded data from the approximate median calculated for the graded data. It is found that the same corrections (to $\frac{1}{2}$ per 1000) hold for grades of 0.1σ, 0.2σ, 0.5σ, and 1.0σ as in the Normal Curve, hence the extent of error introduced by grading appears to be the same up to a considerable degree of skewness. As however η bears a ratio to σ which diminishes slightly with increasing skewness (1.3 per cent when $\kappa=1$), corrections inferred from η will be increasingly though slightly exaggerated. Percentage corrections based on the $\frac{\Sigma_1^4 n}{N}$ criterion will also be exaggerated if the approximate median and the average are more than half the width of a grade apart, but the dependence that can safely be placed on this criterion is generally speaking strictly limited.

To gauge the effect of the proposed correction, examples worked in textbooks have been investigated. The result suggests that the proportion in the four central grades is sufficiently trustworthy as a preliminary indicator for the ordinary frequency group, generally agreeing reasonably well with $\frac{\eta}{h}$. It suggests also that as scientists pledged like others to avoid spurious accuracy and to teach its avoidance, we shall do well, if touching upon the mean deviation at all, not to neglect some consideration of this problem. In fact, if this argument is valid, the conclusion seems unescapable that we must either examine carefully the effect of assumptions made as to graded data, or give up altogether as far as they are concerned the use of the mean deviation.

FORECASTING A LINE BY ITSELF

By H. D. Comer and Ralph J. Watkins, *Bureau of Business Research, Ohio State University*

In all the standard works on the correlation of time series it is assumed that there is only one standard of comparison in gauging the value of correlation results. That standard is the comparison between the standard error of estimate and the standard deviation of the dependent series. The arithmetic mean of the dependent variable is taken as the most probable value in the absence of any other forecast and the standard deviation of the series becomes the measure of the error of estimate. But it is evident that the mean of a time series is a fictitious estimate since it cannot be known until after the actual value for which a forecast is desired becomes available. In actual business practice no business man knows anything about the mean of his series even up to the present. However, he does have a basis from which to estimate the future value of his series, and that is the present value of that series, a perfectly concrete and comprehended value.

There is a certain stability to any economic series and its fluctuations are measured from the present level as a base. The present value represents not only present conditions but the cumulative effect of past conditions in so far as those past conditions influence the present. The price of corn today, for example, is the cumulative effect of past conditions plus present conditions of supply and demand in the corn market.

Then what is the basis for estimating a series, taking into account this stability of the series? Merely this: assume no change at all in the series, *i. e.*, project the series horizontally from its present level; and there is available a ready measure of the error involved in such a process. This is the average change in the series itself for successive intervals of time equal to the period of the forecast. If we desire to forecast retail trade one month in advance, then we find the average month to month change in the retail trade series. In order to secure a measure comparable with the standard deviation, we should compute what might be called the standard first difference, *i. e.*, we compute the root-mean-square of the month to month differences. If this computation yields a standard first difference smaller than the standard deviation, it shows that, on the average, each item in the series is a better estimate of the succeeding one than is the mean of the series. But what is more important is that this same comparison can be made

with the standard error of estimate. If the standard first difference is smaller than the standard error, then on the average, each recorded value of the series affords a better estimate of the next one than is given by the regression equation. In other words, variability in the series itself is less than error in the estimate.

It will be countered that this process begs the whole question of forecasting. This is correct. However, this process does yield a basis for comparison which may be more relevant than comparison with the standard deviation of a time series when short time forecasts are desired. As a concrete example we may take the problem of estimating department store sales in Ohio. In correlating industrial employment with department store sales for the period January, 1922, through August, 1926, allowing a lead of one month to industrial employment, a simple correlation coefficient of +.900 was secured, with a standard error of 2.54 (the series were in the form of index numbers). The standard month to month difference is only 2.39. A forecast of no change at all would have yielded a smaller average error over this period since January, 1922. The standard deviation of the series is 5.84. It is clear here that we have a better basis for comparison in the standard first difference of the series.

When we go beyond a one month forecast, the regression equation based on industrial employment as the independent variable yields better results, as would be expected. That is, the standard first difference increases and is much larger than the standard error of estimate. The results are shown in the table.

MEASURES OF COMPARISON

Time Interval or Lead	r	S_y	$S_{\Delta y}$	σ_y
1 month	+.900	2.54	2.39	5.84
2 months	+.893	2.41	3.06	5.36
3 months	+.844	2.69	4.17	5.01*
4 months	.....	....	4.57	....
5 months	.....	....	5.26	....

* The changes in the standard deviation are due to the dropping of the first items which deviate unusually from the mean. The 5.01 value is the most reasonable value for comparison purposes.

When does the standard first difference, for comparison purposes, cease to be significant as a measure of error involved in the estimate? Clearly, if the standard first difference is equal to the standard deviation, the last value is no better as an estimate of the next value than the mean of the recorded series; it is likewise clear that, if the standard first difference is smaller than the standard deviation, the former is a better comparison measure than the latter in evaluating the correlation

results. We should insist on a standard error materially smaller than the standard first difference; otherwise, we are likely to overestimate the value of correlation studies. The example shown in the table illustrates this. A correlation coefficient of .90 reduces the standard deviation almost 57 per cent; this appears splendid, but further examination of the table shows that the average error would have been smaller had we assumed no change at all.

There is another use to which this concept may be put. If we wish in our forecast to give weight to the stability of the series itself, we can accomplish this by injecting the series itself as an additional independent variable and handle the problem by multiple correlation. An example will illustrate. In the retail trade study, allowing a lead of two months to industrial employment, a simple correlation coefficient of +.893 was secured and the standard error of estimate about the line of regression was 2.41. The series itself two months earlier was injected as a third variable and the coefficient of multiple correlation became +.914 and the standard error of estimate was lowered to 2.17. Thus a distinct improvement is made in the forecast by considering only the stability of the series itself. The injection of the additional variable without an increase in the number of items, of course, reduces the reliability of the coefficient of multiple correlation; however, in the case cited this reduction is more than offset by the increase in the coefficient.

It is evident that this approach to the problem has distinct limitations. If the forecast is for more than a few months ahead, the standard first difference of the series is apt to be so large that it should be possible to secure an outside series which will show a closer relationship. The comparison of the standard first difference and the standard error will reveal this condition.

THE SOCIAL SCIENCE RESEARCH COUNCIL FELLOWSHIP APPOINTMENTS

The Social Science Research Council announces Research Fellowships to be awarded for the year 1928–29.

These fellowships have been established to promote scientific research in the field of the social sciences broadly construed.

Research workers in anthropology, economics, history, political science, psychology, sociology, and statistics (the fields represented in the Social Science Research Council) are invited to apply, and applications of research workers in the closely allied fields of law and education also will be welcomed.

These fellowships are designed to promote the development of research workers rather than to aid in the execution of research projects. Mature scholars seeking aid for well-planned research projects should apply to the Committee on Grants in Aid rather than to the Fellowship Committee. The research fellowships are provided to assist young scholars usually between twenty-five and thirty-five years of age who have already been granted a doctor's degree. Consequently, the fellowships are not offered to assist graduate students to complete research work in order to meet the requirements of a graduate degree, nor are they designed to assist scholars in completing local problems of research which may be financed from other sources.

The amount of the stipend will depend upon the requirements of each applicant and his particular research project. Evidence of exceptional ability in research must be presented by each applicant, together with a detailed outline of a project giving promise of definite scientific accomplishment. If the project requires travel in this country or abroad, the expense of such travel will be taken into account in determining the stipend. The terms of the fellowships may range from several months to as much as two years, depending upon the character and requirements of the problem.

The work of the fellows will be subject to the supervision of the Committee on Social Science Research Council Fellowships and this committee will be available for constructive counsel and advice.

All persons interested in these Research Fellowships are requested to apply to the Secretary of the Committee on Research Fellowships, Professor F. Stuart Chapin, University of Minnesota, Minneapolis, Minnesota. Application forms will be sent on request.

Applications for fellowship appointments in 1928–29 must be in the hands of the Secretary of the Committee by January 15, 1928.

The Fellowship Committee of the Social Science Research Council announces the appointment of the following twenty scholars as research fellows of the Council for the year 1927–28:

NEW APPOINTMENTS

ASHER ACHINSTEIN (Ph.D. Columbia), graduate student, Columbia University.
Project: Time sequences of cyclical phenomena in business with particular reference to the lag between production and prices.
Place of study: New York.

CRANE BRINTON (Ph.D. Oxford), Instructor, Harvard University.
Project: The economic and social status of the rank and file of the Jacobin clubs during the French Revolution.
Place of study: France.

EMILY CLARK BROWN (Ph.D. Chicago), Research Assistant, University of Chicago.
Project: Industrial relations in the printing trades in the United States and Great Britain.
Place of study: New York, Boston, Baltimore.

RUTH L. BUNZEL (Ph.D. Columbia), graduate student.
Project: Social and individual adjustments in a primitive community, with special reference to the Zuni Indians.
Place of study: Zuni, New Mexico.

WALTER L. DORN (Ph.D. Chicago), Instructor in History, Dean in the Colleges of Arts and Literature, Chicago.
Project: The public administration of Frederic II of Prussia.
Place of study: Berlin, Vienna, Paris and London.

WALLACE K. FERGUSON (Ph.D. Cornell), Assistant in Medieval and Intellectual History, Cornell.
Project: A study of the social and political ideas of Erasmus based on his unacknowledged publications.
Place of study: England, France, and Germany.

LEO GERSHOY (Ph.D. Cornell), private tutoring.
Project: The career and influence of Barère during the French Revolution.
Place of study: France.

WILLIAM T. HAM (Ph.D. Harvard), Instructor and Tutor in Economics at Harvard and Radcliffe.
Project: Industrial relations in the building trades in Great Britain and Germany.
Place of study: Great Britain, Germany, France.

MILDRED L. HARTSOUGH (Ph.D. Minnesota), Instructor in Economics and Sociology, Smith College.
Project: A study of economic concentration in Western Germany and the Rhineland, with some reference to its political aspects.
Place of study: Berlin, Hamburg, and Rhenish cities.

LEWIS W. JONES (Ph.D., Brookings Graduate School), publication, writing and economic research, Foreign Policy Association.
Project: Studies in the export of British capital since 1900.
Place of study: Cambridge and London, England.

LEONARD MANYON (B.A.), Instructor in History, University of Michigan.
Project: The guild movement in Italy under the Fascist régime.
Place of study: Italy.

FRANK WALLACE NOTESTEIN (Ph.D. Cornell), Instructor of Economics, Cornell University.
Project: A critical study of European occupational mortality statistics.
Place of study: London, Paris, and Geneva.

JACOB PERLMAN (Ph.D. Wisconsin), Assistant Professor of Economics, Northwestern University.
Project: The development of the brotherhood of locomotive engineers with special reference to the transition to the new unionism.
Place of study: Cleveland, Ohio.

JAMES K. POLLOCK, JR. (Ph.D. Harvard), Instructor in Political Science, University of Michigan.
Project: The use of money in English, French, and German elections.
Place of study: England, France, and Germany.

FREDERICK S. RODKEY (Ph.D. Illinois), Associate Professor of History, Miami University, Oxford, Ohio.
Project: British interests and policies in the Near East, 1821–1878.
Place of study: London, Near East, Vienna, and Paris.

MAX J. WASSERMAN (Docteur en Droit, University de Lyon, France), Associate in Economics, University of Illinois.
Project: The effect of monetary and credit inflation in France on some aspects of French business enterprise.
Place of study: Paris and Lyons, France.

HELEN L. WITMER (Ph.D. Wisconsin), Assistant Professor, Social Hygiene Research, University of Minnesota.
Project: Some effects of the English social insurance acts on pauperism.
Place of study: London.

REAPPOINTMENTS

NORMAN E. HIMES (M.A. Harvard), Instructor in Economics and Sociology, Cornell College, Mount Vernon, Iowa.
Project: The history of the birth control movement in England with special reference to the development and work of the birth control clinics.
Place of study: England.

WILLIAM JAFFÉ (Docteur en Droit, Paris), Tutor in French and Economics, College of the City of New York.
Project: The Industrial Revolution in France.
Place of study: France.

HEINRICH KLÜVER (Ph.D. Stanford), Instructor in Psychology, University of Minnesota.
Project: The eidetic type: field studies in various American communities.
Place of study: Columbia University.

PROGRESS OF WORK IN THE CENSUS BUREAU

DEFINING ENUMERATION DISTRICTS

The Geographer's Division of the Bureau has begun defining and mapping the enumerators' districts for the census of 1930. This work has never been properly done at any previous census, mainly because it was not begun in season. It could not be so long as the Census Bureau was a temporary organization called into existence only a few months before the time for taking the census.

But even in 1910 and again in 1920 after the Bureau had been made permanent the work of laying off and mapping the districts was too long deferred, with the result that the task of completing or perfecting the plans in many cases had to be left to the inexperienced local supervisor, so that there was often much confusion, particularly in cities. An enumerator might find that his district had nobody living in it, or he might find that it had a population greater than he could enumerate within the time limit. Again he might begin canvassing a particular street and perhaps make considerable progress before he discovered that it had been covered by another enumerator. In other cases an area might not be covered by any enumerator. To be sure, errors of this kind were not general and when they occurred were normally discovered and corrected, but not without considerable confusion and delay. To prevent such misunderstandings it is necessary that each district be carefully determined and defined well in advance of the enumeration.

It is by no means a simple or small undertaking to divide the territory of the United States into 100,000 or more districts adapted for the purpose of the census enumeration of population and farms. It is necessary that every enumeration district should be carefully plotted and described, taking into account the congested areas where the political subdivisions must be further subdivided, and following in all such cases so far as possible, natural division lines, such as streams, or existing artificial boundary lines, such as railroads, canals, and highways. Moreover, to know how much territory an enumerator should cover, a study must be made of the topography, roads, and transportation facilities, the sparsity or density of settlement in each division, as well as the number of farms. Even the nationality of the population must be taken into account since the presence of a foreign population unable to speak English adds to the difficulties of the enumeration. All this requires a great deal of correspondence with local people; and the Bureau plans to send a letter of inquiry to some well informed citizen in every township or other minor civil division in the United States, of which there are approximately 65,000. The Bureau must have a record of all changes in the boundaries of political divisions, information which can only be secured from city and county officials. County maps must be secured wherever possible showing the political subdivisions and other detail. For every city or other incorporated place that is too large to be covered by one enumerator maps must be secured showing the blocks and streets.

Each supervisor will be supplied with a map showing the enumeration districts under his supervision, together with a description of each district, giving the population and number of farms as reported in 1920, as well as the present estimated population; and in rural districts also the number of farms as reported in 1920 and estimated present number. Each enumerator will be supplied with a description of his district, together with an outline map showing clearly the territory he is to cover.

The Bureau is planning to make a separate enumeration district of every incorporated place not large enough to be subdivided. In some of the larger cities the Bureau is establishing enumeration districts by blocks. Where that is done the same enumerator may cover several districts or blocks. The use

of the block as a district will make it easy for the supervisor to check the returns and make sure that the city has been entirely covered. It will also eliminate the danger of overlapping and of omissions. It further permits plans to be made that can be easily readjusted if changes in political boundaries occur immediately preceding the census date.

RECORD BOOK OF BUSINESS STATISTICS

As the study of business trends has increased, there has developed a demand for monthly figures on all the important items running back to pre-war years. In the *Survey of Current Business*, this demand was met at first through publication of tables from time to time giving data on such items month by month back to 1913 or even further. But such tables, scattered through different volumes, were not readily accessible for further reference. So it was decided to issue a *Record Book of Business Statistics*, in which might be collected in one place the important statistical data from pre-war years, grouping together all figures on the same industry.

The first section of the *Record Book of Business Statistics* has now been issued, covering the textile field. This pamphlet of 48 pages gives monthly or quarterly statistics of the principal current data on textiles, comparable with those published in the *Survey of Current Business*, but extending back to 1909, where available. This booklet thus enables any reader of the *Survey of Current Business* to obtain comparable figures prior to 1927 on any data published in the *Survey*, for which figures have been gathered at earlier periods. A complete description is given of each series of figures and the bulletin also contains text matter showing the actual uses of such statistics by business firms. This booklet may be obtained from the Superintendent of Documents, Government Printing Office, in Washington at the price of ten cents per copy. The sales have been so large that a third printing has been necessary.

The second section of the *Record Book* covers Metals and Machinery. This will probably be published in January.

MARRIAGE AND DIVORCE

The annual census of marriage and divorce was completed somewhat earlier this year than last and the preliminary totals for 1926 were given out in a press summary released for use on October 17. The final report, presenting the statistics for divorces classified according to cause, party to whom granted, number of years married, whether or not contested, and number of minor children affected, is expected from the printer early in January.

The whole number of marriages performed in 1926 was 1,202,079, as compared with 1,188,334 marriages in 1925, representing an increase of 1.2 per cent. The number of marriages per 1,000 of the estimated population in 1926 was 10.26, as compared with 10.30 in 1925 and 10.36 in 1924.

The number of divorces granted in the United States in 1926, according to the preliminary figures, was 180,868, as compared with 175,449 divorces in 1925, representing an increase of 3.1 per cent. The number of divorces per 1,000 of the estimated population in 1926 was 1.54; in 1925, 1.52; and in 1924, 1.50.

Annulments were reported to the Census Bureau for the first time in the inquiry covering the year 1926. The whole number of annulments reported was 3,823, including 1,282 in California, 964 in New York, 204 in Illinois, 130 in Oklahoma, 103 in Texas, and 101 in Michigan.

J. A. H.

RECENT DEVELOPMENTS IN THE BUREAU OF LABOR STATISTICS

The most important publication of the Bureau during the last quarter relates to the revision of its index numbers of wholesale prices (Bulletin 453). The base has been changed; 1913 no longer equals 100, as the average of prices of the year 1926 is now taken as the base or 100. The weighting period has gone through two changes—first, a single year is no longer considered a base but an average of three years, wherever possible, is taken as the base. Wherever this is not possible then the averages of the two Census years 1923 and 1925 are used as the weighting base. In other words, instead of 1919 being the weighting base and 1913 being the price base, the new index is based upon 1926 as the price base and an average of two and in some instances three years is taken as the weighting base. The number of articles has been increased from 404 to 550. Automobiles and tires, agricultural machinery, sewing machines, traveling bags and suitcases, are among the things that have been added. Several articles no longer important, such as clay worsted suitings, have been dropped.

The new index numbers differ somewhat from the old, as is to be expected from the change in articles and the change in weights. Converting the old general index to the same base period as the new (1926 = 100.0) the following differences are noted: The old series for the year 1923 is 101.8 and the new is 100.6. For 1924 the figures are: Old series, 99.1 and new series, 98.1. For 1925 the old series is 105.1 and the new series is 103.5.

In the 55 months from January, 1923, to July, 1927, the widest divergence in any one month is 2.5 points. In 27 of the 55 months the divergence is 1 point or less, while in 4 months the two series coincide exactly. In all but 11 months of the period under review the new index number is lower than the old, and in only 7 months is it higher.

The change of base from 1913 to 1926 does not affect the Bureau's work on retail food prices, cost of living, nor such wage indexes as it compiles. The change affects the wholesale price index only.

The Handbook of Labor Statistics has proven very popular and is exceedingly convenient in that it brings together in one volume the results of the Bureau's activities along all lines over a series of two years, and in some instances goes back much farther than that.

The July issue of the *Monthly Labor Review* contained a very important contribution on the subject of "Why Workers Borrow," also on "Immigration Restriction and the 'Scarcity' of Domestic Servants."

The cream of the results of the Bureau's recent field survey of welfare activities is being published monthly by topics in the *Monthly Labor Review*. The July issue contained an analysis of establishment benefit associations assisted by the company as found among the 430 establishments covered in the survey. The August number contained an article on "Welfare Work in Company Towns," and the September issue had one on "Indoor Recreation for Industrial Employees" as found among the plants investigated.

The results of the Bureau's study of State and municipal employee retirement systems is likewise being published piecemeal in the *Monthly Labor Review*. The systems of Pennsylvania were reviewed in the August issue and the New York and New Jersey systems are discussed in the September number.

A report to the Secretary of Labor by the Commissioner of Labor Statistics for the year ending June 30, 1927, is the first attempt made in years to cover fully and in considerable detail all of the various activities of the Bureau.

ETHELBERT STEWART

STATISTICAL DEVELOPMENTS IN CHINA

The Nankai University Committee on Social and Economic Research is the first university organization of its kind in China with the purpose of meeting the ever-growing needs for exact and impartial determination of facts which bear upon economic, social and industrial problems. While it seeks only to determine facts, its investigations are to be carried on in such manner and under such supervision as to make its findings carry conviction to liberals and conservatives alike.

In general, the work of the Committee will deal with important economic, social and industrial topics of the day regarding which quantitative analysis is feasible, with the object of placing as much as possible of the present-day discussions on a basis of fact, as distinguished from more or less partisan opinion. The problems listed below are those which the Committee is undertaking this year, and may be taken to illustrate the probable range of its activities hereafter.

I. Rug Industry in China: an economic and social survey. This is the first of the series of studies of the typical industries in China which the Committee proposes to undertake.

II. An inquiry of the family budget of the working classes in Tientsin, with the purpose of providing some basic data for the study of the standard of living of the working classes in Tientsin.

III. Compilation of an index number of the commodity prices at wholesale in the Tientsin market. The Committee is collecting price data of 100 commodities selected on the basis of the extent of their market in Tientsin.

IV. A study of the fluctuation of the physical volume of foreign trade in China, together with an analysis of the changes in its character and distribution from period to period. For this study, the Committee undertakes to construct an index number of foreign trade in China from 1867 to 1926 by the "Ideal" formula as developed in Fisher's *The Making of Index Numbers*.

In addition to the preceding problems, the Committee is compiling two

monthly index numbers of foreign exchange rates from 1911 to 1927—one for Shanghai and one for Tientsin.

The Committee has, at present, a staff of sixteen full-time members: a Director of Research, eleven investigators making investigations in person, and four in the Office engaged in computation and statistical work.

ANSWER TO THE REVIEW OF "RENT LEVELS AND THEIR CAUSES"

To the Editor of the Journal of the American Statistical Association:
Sir:

I should like to call attention to certain features of the review of my *Rent Levels and Their Causes* by Miss Margaret Loomis Stecker in the June issue of this JOURNAL. Miss Stecker's chief criticism of my study is based on an alleged failure on my part to weight the "for rent" averages in proportion to the number of four, five, and six-room houses used in the computation of the index number of rents. This criticism is unjustified because, with but one exception, I used the medians instead of the means precisely because I wished to avoid the error of unequal weighting. Footnote 5 on page 12 reads as follows:

> The median is of particular advantage in this study inasmuch as no attempt has been made to weight the arithmetical average for each year in proportion to the rent asked for four, five, and six-room dwellings. The use of the median makes it a practical certainty that it is the rent of the five-room dwellings that is compared throughout this period. This assumption is supported by the results of a comparison of the distribution of the four, five, and six-room dwellings in the advertisements for the spring of 1913 with that of the spring of 1924. The figures follow: March–April, 1913, four rooms 868, five rooms 846, six rooms 899; March–April, 1924, four rooms 809, five rooms 737, six rooms 501. Rents in the spring of 1924 were second highest of the post-war period. The spring of 1924 was chosen in preference to the highest rent period, that of the spring of 1921, because of the relative smallness of the sample of the latter year.

In the one instance (pages 10–12) in which I used *average* rents, I pointed out (footnote 1, page 10) that: "The averages thus computed . . . are consistently between $2 and $3 higher than the median . . . which by their nature are free from the extremes. . . ." No serious error would have been committed even if I had used the unweighted means.

In the first paragraph on page 12, the careful reader will also discover that I was not in error when I compared my rent index with that of the Bureau of Labor Statistics on an arithmetical scale, the two having different base years. The purpose of that diagram (II, page 13) was to show that, since 1913, "Rents in Pittsburgh, as disclosed in my study, have gone much higher than one would be led to believe from the government data," because, among other things, by 1917, the Bureau of Labor Statistics base year, "rents in Pittsburgh had risen some 50% above 1913."

Finally, may I be forgiven for the mechanical error in the ratio chart on page 30, where, to quote Miss Stecker, "the distance from 100 to 200 is considerably [?] greater than the distance from the 200 to 400, which, of course, should be

exactly equal." However, Miss Stecker has failed to observe that but for this mechanical error my rent curve would have showed up even to greater advantage than is the case at present.

The writer would be the last one to contend that the conclusions relating to *Rent Levels in Pittsburgh, Pennsylvania, and Their Causes* would not bear further refinements. But he is not convinced that Miss Stecker has discovered any vital errors in his procedure.

J. M. GILLMAN

MISCELLANEOUS NOTES

Meeting of the San Francisco Chapter.—The Fourth Dinner Meeting of the San Francisco Chapter of the American Statistical Association was held at the Commercial Club on the evening of Friday, September 23, 1927. Thirty-six were present. Professor A. H. Mowbray of the University of California presided.

A report of the Nominating Committee was presented, and the following officers elected: President, A. H. Mowbray, University of California; Vice-President, Holbrook Working, Food Research Institute, Stanford University; Secretary-Treasurer, recommended to the National Association for appointment, Robert W. Bachelor, Pacific Telephone and Telegraph Company.

The topic for discussion was "Problems of Presenting Statistical Results," the subject being suggested by the Presidential Address of Leonard P. Ayres entitled "The Dilemma of the New Statistics" and more particularly by his statement, "The value of statistics is directly and almost completely dependent on the quality of the presentation."

The discussion was opened by Norman J. Silberling of the Silberling Business Service, who spoke of his experience in interpreting business barometers to executives in varied lines. He stated that a business index or barometer is a composite of several series and calls for complicated adjustments for seasonal fluctuations, amplitude, and trend. When these series are combined into one line, business men are confused, because they are not familiar with technical terms, such as standard deviation. This difficulty may sometimes be avoided by selecting series which have about the same amplitude of fluctuation, and taking a mathematical average, expressing the results in units of percentage deviation. Business men are confused by the term normal which should be more clearly defined. Many also are confused by the fact that normal is shown as a horizontal line while the particular business of the executive may show a pronounced growth from year to year. Often such an executive is more interested in the rate of normal growth than he is in the cycle swings. Dr. Silberling has found it less confusing to show the index expressed in deviations from a tilted line which approximates the trend of the index as a whole. For experimental purposes work has been done for certain cities using as the trend an average of certain indexes of city growth, such as postal receipts and telephone installations, and upon this superimposing the cycle curve made up of car loadings and financial series. This method has certain technical objections which may be removed by showing a cycle chart with the trend put in, and below showing the same cycles as measured from a horizontal line. Lead and lag relationships are difficult to indicate since if more than two curves are shown on a chart the reader is confused, and finds it difficult to grasp such a relationship. To obviate this difficulty it is desirable to express the forecast in terms of probable range. This range (one standard error) is indicated by a light red shading and the actual index shown by a heavy black line. Dr. Silberling closed by stressing the need for standardization in methods of presenting data.

The second speaker, H. D. Gidney, Statistician of The Pacific Telephone and Telegraph Company, approached the subject from the standpoint of the statistician interpreting the internal affairs of the company to the business executives. He divided the problem into three parts: (1) Gathering the data that the business executive needs; (2) Preparing the data to tell a true and concise story; and (3) Presenting the material in a way that the executive will understand.

Most of the d[illegible] on internal operations may be divided into three classes: (1) Problems of analysis of current operations; (2) Problems of economic research; and

(3) Problems in anticipation of future requirements. Mr. Gidney stressed the fact that a thorough knowledge of the industry was more important than a training in statistical technique, and that a standardization of methods of analysis for a particular industry had many advantages making for economy of effort and accuracy of results. It was his experience that the presentation must be adapted to the personal tastes of the executive who was to use the data and should combine graphics, tabulation, and narration.

The third speaker was Oliver P. Wheeler, Manager of the Division of Analysis and Research, Federal Reserve Bank of San Francisco. Mr. Wheeler stressed the need for a statistical interpreter who could properly interpret the language of the technician to the business executive who was to make use of the studies. He outlined the history of management and control, from the earlier times of haphazard thinking to the more advanced forms of management where reflective thinking, based upon scientific analysis, controls. The statistician must sell the idea of statistics to the management, and to do so must be persistent, resourceful, and diplomatic. Executives who have had years of experience with certain types of statistics feel qualified to discuss them and resent a new interpretation which they may not understand, especially if made by one who has no specialized knowledge of the business. The appearance of mystery as to methods should be avoided, and the results stated in the simplest non-technical terms. An example of statistical interpretation was cited from the Federal Reserve Bank where research assistants specialize in particular fields, and in collaboration with the statistician, prepare reports on specific phases of their work. Each report, when complete, is edited by the manager, who does not specialize in any field. The work then goes to the Assistant Federal Reserve Agent who further edits it from the standpoint of the reader who is not possessed of technical knowledge. After this it is returned to the research assistant who prepared it to see that no misstatements of fact have crept in, and that no vital points have been left out.

Mordecai Ezekiel of the United States Department of Agriculture commented on problems of presenting results of agricultural research, involving complicated technique, to residents of agricultural regions. He found it desirable to show each curve on a separate grid, one above the other. Leads or lags could then be pointed out by means of colored arrows from one curve to the other, with an arrow from the end of the forecasting curve to the space within which the second curve might be expected to fall.

Dr. Joseph S. Davis of the Food Research Institute, Stanford University, suggested that the statistician should find out for himself what the statistics really mean in a way of economic or business significance before attempting to interpret the meaning to others; and that oftentimes the statistician is not clear as to the real meaning of a particular study or tabulation.

Dr. H. E. Erdman of the University of California suggested that in circulating preliminary studies it was advisable to keep the style technical so that only technicians could understand the significance. Thus it would draw out real criticisms which might often reveal basic assumptions which were doubtful and which should not be given a wide circulation to the general public.

The meeting adjourned.

PERSONAL NOTES

During August and September, Dr. Alfred Manes, Professor of Insurance at the *Handelshochschule*, Berlin, Germany, visited a number of insurance companies in New York City, in Hartford, and in Philadelphia. His purpose was to collect material for the fifth edition of his *Versicherungswesen*. This work is shortly to be published in three volumes, presenting a world-wide review of private and social insurance.

During his visit in New York he saw the executives of the companies and collected a great deal of material, including the more recent statistics of the business. He spent one week in Hartford, studying the insurance business in that city, a week in Philadelphia, where he visited the Wharton School of Finance, and a week in Washington, D. C. While in Washington, Professor Manes lectured before the Brooking School, Institute of Economics. Dr. Manes' previous visit to this country was in 1903, when he attended the New York meeting of the International Congress of Actuaries. His interest in statistics prompted him to inquire into the growth of statistical activity in American business establishments, especially in the insurance business. He expressed particular satisfaction at the progress which the American Statistical Association had made, both in numbers and in influence, since his last visit twenty-four years ago. As General Manager of the German Society for Insurance Science, Dr. Manes will, no doubt, be able to present to his German colleagues a picture of contemporary activities in statistics and insurance in this country.

Professor James W. Glover of the University of Michigan has returned from a trip abroad this summer. He was a delegate to the World Population Conference held in Geneva the first week in September.

Dr. Edwin W. Kopf, Assistant Statistician of the Metropolitan Life Insurance Company, has recently been elected a Fellow of the Royal Statistical Society of London.

Mr. Roswell F. Phelps, Director of Statistics in the Massachusetts Department of Labor and Industries, has left this country for Japan where, in connection with addresses before Japanese business men, he is to exhibit a large assortment of material, including samples, photographs, slides, films, and descriptive catalogs, of New England products which should find a ready market in Japan. He expects to return to Boston about February 1st, leaving Tokyo early in January. While in Japan his headquarters will be at the Imperial Hotel in Tokyo and his cable address there is "Exhibitor—Tokyo."

Dr. Joseph M. Gillman has resigned from the University of Pittsburgh to become Economist for the Cavendish Trading Corporation, 180 Madison Avenue, New York City.

Mr. Charles E. Baldwin, the Assistant Commissioner of Labor Statistics, has returned from his trip to Europe, where he was studying certain facts in regard to woolen and worsted textiles.

Mr. Lindley D. Clark, the Bureau of Labor Statistics' legal expert, has resigned from the Bureau to accept a position with the United States Employees' Compensation Commission in connection with the administration of the Federal longshoremen's and harbor workers' compensation act, which was placed in the hands of that Commission.

Dr. Marcus Nadler, in September, resigned his position with the Division of Research and Statistics of the Federal Reserve Board and accepted an appointment as Assistant Professor of Economics at New York University, and Director of the Institute of International Finance.

Miss Aryness Joy, Assistant Professor of Economics, at Mount Holyoke College, who, during the summers of 1926 and 1927, was engaged in studies of seasonal variations for the Division of Research and Statistics of the Federal Reserve Board, has

accepted a temporary appointment with the Federal Reserve Board, extending from February to September, 1928, in order to continue these investigations. She has been granted a leave of absence from Mount Holyoke College during that period.

A communication from Norman Crump indicates a change of address to 11A Bishopswood Road, London, N. 6, England.

ADDITIONAL COMMITTEE APPOINTMENTS, 1927

A new committee has been instituted. This is the *Committee on Classification of Members:* Edwin W. Kopf, *Chairman;* Donald R. Belcher, Harry C. Carver, Robert E. Chaddock, Joseph A. Hill, Willford I. King, and Warren M. Persons.

MEMBERS ADDED SINCE SEPTEMBER, 1927

Abramhoff, Israel, Manager, Department of Commercial Research and Statistics, Albert Frank and Company, 11 Avery Street, Boston, Mass.

Arnott, Herman J., Sales Analyst, The J. R. Clark Company, Aldrich and 2 Streets, Minneapolis, Minn.

Baker, Franklin, Jr., Franklin Baker Company, Hoboken, N. J.

Baude, Walter A., Instructor, College of Engineering and Commerce, University of Cincinnati, Cincinnati, Ohio.

Beale, Elizabeth, Student, Sandwich, Mass.

Bechler, W. M., 383 Grand Avenue, Akron, Ohio.

Bowles, Herbert R., Student, Columbia University, New York, N. Y.

Butler, Roy G., Assistant Engineer of Subway Design, The Port of New York Authority, New York, N. Y.

Crossley, Archibald M., Crossley, Inc., 25 West 43 Street, New York, N. Y.

Crumb, Joseph A., Supervisor of Research and Statistics, State Banking Department, San Francisco, Calif.

Davis, George E., Statistician, North Jersey Transit Commission, Jersey City, N. J.

Davis, Harold T., Assistant Professor of Mathematics, Indiana University, Bloomington, Ind.

Davis, Rebecca R., Business Men's Commission on Agriculture, 247 Park Avenue, New York, N. Y.

Dow, Douglas, Detroit Edison Company, Detroit, Mich.

Ebling, Walter H., Agricultural Statistician, Crop Reporting Service, U. S. Bureau of Agricultural Economics, Madison, Wis.

Elliot, Mildred M., Graduate Student, University of Illinois, Urbana, Ill.

Evans, Dr. George H., Jr., Instructor, Johns Hopkins University, Baltimore, Md.

Folger, A. Douglas, Assistant Chief Statistician, E. R. Squibb and Son, 80 Beekman Street, New York, N. Y.

Gadeholt, Bjorn, Statistician, Northwestern Electric Company, Portland, Ore.

Gillies, Robert C., Statistician, American Telephone and Telegraph Company, New York, N. Y.

Good, Alexander C., Consulting Actuary Life Insurance, 1416 Chemical Building, St. Louis, Mo.

Hixson, J. H., County Superintendent of Schools, Wapakoneta, Ohio.

Holden, Thomas S., F. W. Dodge Corporation, 119 West 40 Street, New York, N. Y.

Howard, Thomas W., Assistant Manager, Department of Manufactures, Chamber of Commerce of the United States, Washington, D. C.

Jacobson, Bertha, National Industrial Conference Board, 247 Park Avenue, New York, N. Y.

Jones, Marion H. (Mrs. J. M.), Psychologist of Public Schools, Scranton, Pa.

Karpf, M. J., School for Social Work, 210 West 91 Street, New York, N. Y.

Knick, George W., Instructor, Marquette University, Milwaukee, Wis.

Letchworth, William P., Statistician, Wm. Cavalier and Company, Insurance Exchange Building, San Francisco, Calif.

Lohr, Louis W., Professor at Howard College, Birmingham, Ala.

McAlpin, David, III, Statistician, Clark, Hodge and Company, 51 Wall Street, New York, N. Y.

Merriwether, Duncan, Student, Columbia University, New York, N. Y.

Moulton, H. G., Director, The Institute of Economics, 26 Jackson Place, Washington, D. C.

Ness, Marie M., Statistical Research, Department of Botany, University of Minnesota, Minneapolis, Minn.

Nutt, Alice S., U. S. Children's Bureau, Department of Labor, Washington, D. C.

Oak, Vishnu V., Professor at Wilberforce University, Wilberforce, Ohio.

Palmer, James L., Graduate Student, University of Chicago, Chicago, Ill.

Rahlff, John H., Traffic Analyst, The Port of New York Authority, New York, N. Y.

Rice, Frances R., Student, Hewlett, Long Island, N. Y.

Robinson, Seth B., Jr., Statistician, The White Company, 842 East 79 Street, Cleveland, Ohio.

Rose, Mabel E., Federal Reserve Bank, New York, N. Y.

Suehsdorff, Adolph, Jr., Financial Statistician, International Acceptance Bank, 52 Cedar Street, New York, N. Y.

Stoddard, George D., Assistant Professor of Psychology and Education, University Hall, Iowa City, Iowa.

Van Arsdale, Marjorie, Statistician, Board of Education, Chicago, Ill.

Williams, Lloyd A., Statistician, Carnation Milk Products Company, Oconomowoc, Wis.

REVIEWS

Interpolation, by J. F. Steffensen. Baltimore: The Williams and Wilkins Company. 1927. ix, 248 pp.

This book is a translation by the author from the original which was published in Danish in 1925. English readers have cause to be grateful both to the author and publishers for their efforts to make the work available to English-speaking students because it is undoubtedly the best textbook on interpolation since the publication of Boole's *Finite Differences*. It is also particularly valuable because it brings the work on interpolation right up to date. Professor Steffensen is one of the leading contemporary contributors in this subject and is eminently fitted to present the work of others as well as his own. He is correct in his view that a non-rigorous point of view is dominant in most textbooks on interpolation. This cannot be said of the present book; little beyond the calculus is required to follow the arguments, and the treatment follows the lines of strict analysis. The author clearly distinguishes between the operation of calculating the value of a function previously defined, and that of inserting values in an interval where the function is not defined. The weakness of most interpolation formulas lies in the fact that no attention is paid to the error involved in stopping at a given number of terms. Professor Steffensen has laid great stress upon the remainder-terms, and has attempted to provide them in simple form in connection with the various interpolation formulas considered.

Displacement-symbols are first considered with regard to their properties as operators, and their relation to descending, ascending, and central differences is pointed out. The author does not neglect to call attention from time to time throughout the book to the analogous symbols and operations in the differential and integral calculus. Article three deals with divided differences after which the well-known interpolation formulas of Newton, Gauss, Sterling, Bessel and Everett are developed with particular attention given to the remainder-term in each case. Some twenty pages of interesting applications follow, indicating the method of determining the remainder-term in numerical examples and the limits of error involved in using the formulas. The factorial coefficients and the expansion of factorials, such as the differences of zero, are explained, followed by an interesting treatment of numerical differentiation, that is, the problem of calculating the successive differential coefficients of a function when a table of the function is given. One of the most useful applications of interpolation is to the construction of tables and the author gives this subject its due attention. A few pages follow devoted to the subject of inverse interpolation, and a number of methods are set forth in detail. This important subject usually gets scant treatment in textbooks, in fact, very little is to be found on inverse interpolation in our mathematical journals.

Elementary methods of summation, repeated summation, and the development of the more important summation formulas, including those of Laplace, Gauss, and Euler receive very thorough treatment in the next fifty pages and

again the author lays stress on the error-test and remainder-term in the application of these formulas. The very useful and little known formulas, outside of actuarial literature, of Lubbock and Woolhouse are for the first time carefully set forth in this book. In addition to the usual Lubbock formula the author calls attention to two other formulas of the Lubbock type found by using Sterling's and Bessel's formulas, respectively. These are natural extensions of Lubbock's idea, applying formulas other than that of Newton. The latter developments are sometimes more useful than the formula proposed directly by Lubbock. This was verified in a recent paper by Jenkins,[1] who applied Everett's formula to obtain a so-called Lubbock type. The important formula of Woolhouse is developed and a second type of Woolhouse formula is also given. Again one must feel grateful to the author for bringing out clearly the values of the remainder-terms, and making it possible to arrive at some notion of the error involved in the use of this formula.

The usual textbook formulas of mechanical quadrature are given with illustrative examples, and in addition the formula of Hardy, which does not occur outside of actuarial literature. This is followed by a few pages on the subject of numerical integration of differential equations, hardly enough to be of much practical value to the student who is not familiar with this subject from other sources. The book concludes with an extended article on the symbols of calculus, an article on interpolation with several variables, one on mechanical cubature and an appendix on differential coefficients of arbitrary order. These latter articles will be useful to the advanced student.

The author intends that this work shall be used as a textbook—not a handbook or encyclopedia. It would be more useful in the classroom in this country if, in addition to the illustrative applications, examples were given for the use of students at the end of each main article. The price of the book, $8.00, is also likely to stand in the way of its general classroom use. It is to be regretted that the author did not include a list of references or a guide to the literature of finite differences and interpolation, rather than referring the student to Nörlund's *Differenzenrechnung*. In view of the tremendous advance in applied mathematical statistics in recent years, a book of the kind which Dr. Steffensen has produced, written along rigorous lines and breathing throughout the spirit of the deep scholar, is peculiarly welcome and appropriate. The publishers should be credited for their part in producing a book in excellent printed style in a subject full of difficult symbols.

JAMES W. GLOVER

University of Michigan

Elements of Statistics, Fifth Edition, by Arthur L. Bowley. London: P. S. King & Son, Inc. 1926. xi, 462 pp.

Professor Bowley's *Elements of Statistics* has passed to its fifth edition. The fact that a book of this class has been in constant use for over twenty-five years, outgrowing four editions, speaks very loudly for its excellence, without any

[1] "On a Central Difference Summation Formula," by W. A. Jenkins. *American Mathematical Monthly*, vol. 32, pp. 398–403.

further comment. So many editions have enabled the author to eliminate errors, to make additions, and to bring the material up to date. As a consequence the fifth edition is very little different from the fourth edition. And to the many persons familiar with the fourth edition little needs be said concerning the fifth.

In a Note to the Fifth Edition on page viii the author says that a considerable number of corrections in the print have been made, that a note has been added on page 433 concerning a question of recent discussion, and that an appendix (11, page 454) has been added giving a simpler derivation of the formula found on page 429.

The note on page 433 concerns application of a test for correspondence between observed data and a selected formula as applied to double or manifold tables. The note merely states that the procedure is different according as the totals of lines and of columns are supposed to be given or not, and refers the reader to *Economica* and the *Statistical Journal.*

The new appendix on page 454 gives an additional proof of the test formula for probability of the occurrence of a given set of errors or differences between observed values and the values to be expected from a given frequency curve. This new proof is due to Professor Pearson and uses the multinomial theorem instead of the former proof in Chapter X. The proof in Chapter X is also due to Professor Pearson.

Among the corrections made the writer has run across a few. For example, on page 357, line 2, the words "a particular value" have been inserted. At the bottom of page 359 the product of the two series obtained by expansion is replaced by a three-line statement in regard to the expansion. The result, following on page 360, is, of course, unchanged.

The writer has noted a few errors still remaining in the print. For example, on page 282, line 22, C_{pn+x} should read ${}_{pN}C_{pn+x}$. On page 222, in the fourth column of the table, Δ_1^3 should be Δ_2^3. These are obvious errors of print. On page 226, just below the table, the statement is made that "here the successive differences diminish regularly and the sixth difference is not greater than .000 0001." Upon carrying the table farther this statement appears not to be true. Also, if eight-place logarithms are used, it is at once evident that the statement does not hold as to sixth differences. However, this does not vitiate the conclusions drawn in so far as they relate to the numbers 20 to 27, inclusive.

For those not familiar with previous editions of this book it may be well to add a few words as to the manner of presentation of the material, and the field covered.

The book is divided into two parts. Part I deals with General Elementary Methods. Part II deals with Applications of Mathematics to Statistics.

In Part I the introductory chapters deal with the scope and meaning of statistics, general method of investigation, and definitions. The connection between statistics and the physical sciences, biology, meteorology, social science, economics, etc., is noted. Our attention is called to the fact that great numbers and the statistical constants derived from them have great inertia. It is due to this fact that statistical measurements become valuable. In this introductory matter we are also told that the chief practical use of statistics is that of making

comparisons. Difficulties in the collection of data are discussed and methods for overcoming them suggested.

Chapter V is on tabulation. The method used for tabulating classes in frequency tables, in the opinion of the writer, is undesirable. An example is shown on page 86. Here the first age-class of married men is given as 15–20, the second as 20–25, the third as 25–30, etc. It is impossible to tell whether the age of 20 years belongs in the first class or the second. Some tabulators consistently regard the upper limit of each class as tabulated as belonging in the next class following. Others regard the lower limit as belonging in the next class preceding. Still others would regard the age of 20 years, in this table, as belonging half in one class and half in the next.

In such a table as that on page 69 where the first class of daily wages is stated to include as much as \$0.25 and less than \$0.35, the second class as much as \$0.35 and less than \$0.45, the meaning is clearer. Here, however, the mid-point of the class is not known precisely since the largest wage belonging in each class is unknown.

This is not a fault peculiar to the book being reviewed. The suggestion is merely offered as a general criticism of many statistical tables found in print.

Chapter V contains quite a full treatment of averages, including arithmetic average, weighted average, mode, median, and geometric mean. Other averages such as harmonic mean, and quadratic mean; are not discussed. At the end of the chapter a satisfactory average is stated to have the following characteristics:

"If there is a type it shows it; it gives due influence to extreme cases; it is not easily affected by errors or much displaced by slight alterations in systems of calculations; and it is easily calculated."

In Chapter VI the usual measures of dispersion are treated, as is also Professor Gini's mean difference. The quartile measure of skewness is shown. A number of examples of the application of averages are added at the end.

In Chapter VII a number of valuable suggestions on the construction and meaning of graphs are to be found. At the beginning of the chapter the author says: "The two main methods of elementary statistics which ought to be understood by all students or officials who handle figures, which are easily within the grasp of all independently of mathematical training, but are generally misunderstood or ignored by the uninterested or the uninitiated, are the method of averages and the method of diagrams or the graphic method." The mind can grasp but few numbers at a time. A good average provides a single number as a type representative of many. The good diagram or graph provides a representation of the entire set of numbers in a manner such that size and manner of variation is conveyed to the eye at a glance.

The idea of graphical representation is made to depend on use of coördinate axes as in analytic geometry. This is logical, since most graphs actually employ abscissas and ordinates.

A good example of smoothing is given in connection with exports of British and Irish produce. A more mathematical method of smoothing is given later in the chapter on interpolation at the end of Part I.

Considerable space is used in studying the proper interpretation of the dia-

grams, seasonal influence, trend, elimination of fluctuations, etc. The last five pages of the chapter are devoted to logarithmic curves. To one familiar with the now common graph paper ruled to logarithmic scale the treatment here given seems awkward.

Chapter VIII, on accuracy, is well worth reading. In connection with Chapter IX on index-numbers some good American text should be read.

Chapter X deals with interpolation in a necessarily brief manner. However, it makes excellent reading. One is impressed with the idea that more attention should be given to the calculus of finite differences in our college curricula.

Part II is more for the statistician having a good elementary knowledge of mathematics. Part I has given material which the high-school graduate should be able to read. Part II requires at least the first two years of ordinary college mathematics. Some writers, notably Yule, have attempted to treat statistics without differential and integral calculus. This necessarily handicaps the treatment and frequently demands such statements as: "The proof cannot be given within the limitations of the present work." Professor Bowley's Part II is not handicapped in that way. He makes constant use of the work of Professor Karl Pearson, Professor F. Y. Edgeworth, W. P. Elderton, and others. He lays but little claim to anything wholly original. Nevertheless, it is a fine piece of work to have gathered together related portions of the work of so many men and placed them in a single book so as to make a continuous, logical whole.

It would be difficult to criticize the work of Part II unless one wishes to criticize in general the work of the whole English school of statistics. It would be interesting to have the work of Charlier and the Scandinavian school alongside for comparison and study.

Every person who has to handle statistics intelligently, and every person interested in the fundamental ideas involved in the practical use of statistics and the limitations of the methods employed should make himself familiar with the contents of Part II or its equivalent.

Like Part I there are ten chapters in Part II. In the introductory paragraphs it is stated that in the statistical field there are many classes of problems in which mathematical treatment of a special nature is requisite. Those problems which are fundamental and of direct interest to students of economics and allied sciences are covered. Of course the same methods may be almost universally used.

The author divides the problems into three groups: (1) The systematic description of groups, (2) The measurement of relationship between phenomena, and (3) The measurement of the precision of results obtained by a process of sampling.

One cannot go very far with the author in pursuit of these problems without realizing the extreme importance of the calculus of probabilities. The rapidly growing importance of statistics, not only to economics, but to many other branches of human knowledge, would seem to make imperative a more thorough study of probabilities in our colleges. Reasonably good texts in English can now be obtained on this subject.

The first chapter deals briefly with frequency groups and curves. The idea of "moments" is introduced with the notation and illustrations of methods for

their calculation. The use of moments in measuring skewness is discussed. The investigator now has three condensed means of comparing the characteristics of two or more groups of variables. First there is an average, then a measure of dispersion or scatter, and now a good measure of skewness of the distribution.

Chapter II is devoted to algebraic probability and the normal curve of error. The equation of the normal curve of error is derived through the well-known binomial distribution of probabilities, finding the limit of $(p+q)^n$ as n increases without limit.

The generalized law of error or law of great numbers follows in Chapter III. The proof by the multinomial theorem is given and followed by Professor Edgeworth's proof.

The thirty pages of Chapter IV take up applications of the law of error with examples.

Chapter V is very brief, dealing with empirical frequency equations. Two pages are devoted to Professor Karl Pearson's system of curves. Professor Edgeworth's method is merely mentioned in one paragraph. Professor Pareto's equation is discussed in two pages and a half. The last page is devoted to Makeham's formula.

The next three chapters are taken up with the theory of correlation, simple, partial, and multiple, together with examples.

Chapter IX introduces the problem of inverse probability and its application to the determination of precision of measurements. The last chapter derives tests of correspondence between observed data and formulas used for theoretical distribution.

The index is much enlarged, being about twice as extensive as that of the fourth edition. It has been said that a book worth writing is worth having a good index. It is a delight to see that this book has one. A personal index is also added which is well worth while.

G. Irving Gavett

Modern Monetary Systems (Translation of *Les Systèmes Monétaires Modernes*), by Bertrand Nogaro. London: P. S. King & Son, Ltd. 1927. 232 pp.

Since 1914, the world has assumed the aspects of a currency laboratory or clinic, in which the economist can find specimens of every kind of monetary pathology known to his science. From this wealth of material a number of studies have already appeared, which have advanced the theory of monetary economics. Among these, de Bordes' account of the reconstruction of Austria will at once be remembered. Governor Franck of the Belgian National Bank and President Schacht of the Reichsbank have described the experiences of their respective countries. Using the laboratory method, but aiming at more general deductions, Professor Rist (now vice-Governor of the Bank of France) wrote *La Deflation en Pratique;* and it is to a certain extent in refutation of this work that Professor Nogaro, of the University of Paris and member of the Chamber of Deputies, has produced *Modern Monetary Systems.*

M. Nogaro's conclusions, and even his premises regarding currencies, are at variance with certain widely held views. For example, he states "external depreciation, . . . and internal depreciation are two phenomena which may be related and may even affect each other; . . . they are essentially distinct phenomena" . . . (page 128).

On the basis of such a premise, he can hardly offer more than a qualified acceptance of the purchasing power parity theory; and in this respect, his conclusions are rather similar to those of M. de Bordes, drawn from the Austrian experience.

But while de Bordes considered that the case of Austria supported the quantity theory of money, M. Nogaro rejects this theory, and in that way joins issue with Dr. Rist. He argues both from the experience of Germany and Czecho-Slovakia that it was the exchange rate which moved prices and the movement of prices which forced the expansion of the currency; and demonstrates that once the exchange was stabilized through convertibility, prices were stabilized pretty nearly regardless of the quantity of money. In our opinion, the subsequent experience of Belgium, France, Italy and Norway tend to support M. Nogaro's argument; without, we may add parenthetically and paradoxically, invalidating the general position of Dr. Rist. In short, while Nogaro, Rist and Cassell, to take three names only, reach somewhat different conclusions from their clinical observations, it appears to us that their differences are to a great extent those of emphasis rather than principle.

For Americans, the greatest interest in M. Nogaro's work will lie in his concrete testimony against the mathematical quantity theory. In this position he is, of course, brought into opposition to the doctrines of Dr. Irving Fisher and J. M. Keynes, whose theories, if not proposals, have obtained considerable acceptance in this country.

Professor Nogaro concludes, therefore, that the external value of a currency depends upon the maintenance of convertibility of the currency rather than upon the limitation of its quantity. These two facts, however, must be related to each other far more closely, we think, than Professor Nogaro seems to admit. The maintenance of convertibility is difficult, and may become impossible, if the quantity of currency in a country is so redundant that a portion of it seeks conversion into a foreign unit in order to find employment. We regard the neglect to relate the principle of convertibility to this aspect of the principle of limitation as a serious omission from a comprehensive and stimulating study of currency questions.

ROBERT WARREN

Cost of Living in New York City, by The National Industrial Conference Board, Inc. New York. 1926. 129 pp.

The appearance of this report is another event attesting a continued, and perhaps growing, interest in family and individual living costs. While workingmen's budgets have been the subject of inquiry as far back as the seventeenth century when Sir William Petty made his estimate of the laborer's cost of living

in England and Ireland, it was not until the World War wrought its widespread economic disturbance that cost-of-living studies attracted general attention in this country and exerted any real influence on practical affairs. No doubt the greatest single impetus given these studies during the war period came from the federal government which, through its highly useful Bureau of Labor Statistics, conducted numerous investigations into living costs to afford a basis for wage adjustment in war industries, especially that of ship building. Not only were actual household expenditures ascertained, but standard budgets were formulated for families and individuals. During the period of rising prices these studies were helpful to wage earners in securing higher wages. It was only natural, therefore, that they should have been welcomed and encouraged by large groups of organized labor and by their sympathizers. When prices took a downward turn, laboring groups lost some of their ardor for cost-of-living facts, but private employers developed a greater interest in them than they had shown previously. The result has been that investigations into living costs have continued unabated. Of the unofficial bodies that have engaged in such investigations in recent years, none has been more persistent than the National Industrial Conference Board, an employers' organization.

Cost-of-living studies may be divided into two classes. One comprises studies that include an inquiry into the income and expenditures of actual families or individuals. In this class belong the investigations made during the period of the World War by the United States Bureau of Labor Statistics and the Bureau of Municipal Research of Philadelphia. The other class comprises studies that do not include the collection of primary information on family or individual income and expenditures but rely on the results of other studies. In this class belong the cost-of-living studies of the National Industrial Conference Board. The report on the *Cost of Living in New York City, 1926*, is the latest of a series of similar reports which this organization has devoted to living costs in various cities in the United States.

In the language of the report itself, the investigation of which it presents the results—

> . . . was undertaken to ascertain the minimum cost of maintaining a fair American standard of living in New York City. An attempt was made to portray living conditions of (1) industrial workers and (2) office workers, and prices in the spring of 1926. The cost of living measured was related to the requirements of (1) a family group; (2) a single man without dependents; (3) a single woman without dependents. The investigation was designed to determine as far as possible the prevailing cost of living at the standard specified. The results are based on a survey of prices necessary to maintain this standard, and not on sums actually paid out by families.

Interest centers mainly in the content of the "fair American standard of living" as defined in the standard budgets adopted by the National Industrial Conference Board. These budgets, as already indicated, were formulated without recourse to another examination of the income and expenditures of actual families or individuals. The results of other studies were drawn upon, and some account was taken of living conditions of workers in various sections of New York City as revealed by neighborhood visits and information obtained from social-

welfare organizations, public utility companies, industrial establishments, and other private business concerns. In a somewhat hasty inspection of these budgets, and a fragmentary comparison of them with other standard budgets, one does not find any distinct departure from standards set by other authorities. If one makes allowances for living conditions peculiar to New York City, it is difficult to determine whether the standard of living provided for in the budgets of the National Industrial Conference Board are higher or lower than, for example, the standards assumed by the United States Bureau of Labor Statistics or the Bureau of Municipal Research of Philadelphia. In other words, the National Industrial Conference Board deserves to be exonerated from suspicion that, as an employers' organization, it has sought to depress the "American standard of living" below the level assumed by other investigators.

If a criticism were to be made of the standards adopted in this report, it would be that they are too much like the standards formulated a decade ago. It is probable that workingmen live better today than they did at the outbreak of the World War and immediately thereafter. Yet the basic information upon which the standard budgets of the National Industrial Conference Board are largely based pertains to past rather than to present conditions of living. The National Industrial Conference Board might well consider the suggestion that it make original inquiries of its own into the living conditions of workers, and repeat these inquiries every five years, so that its standard budgets may be kept in approximate harmony with the changing facts of life.

The reviewer cannot leave the subject of standards of living without some observation on the report's discussion of the size and income of families. In order to counteract the view that the typical workingman's family consists of the two parents and three children under 14 years of age, and that this family is dependent on one wage earner, the report cites statistics, mostly from the federal census, as follows:

> The census shows that in the entire population of the city [of New York] in 1920 there was an average of 1.2 children 14 years of age or under in a family group of 4.4 persons; which means 3.2 persons over 14 years of age per family. The 1920 census also shows that in this average family of 4.4 persons there was an average of two wage earners. The New York State Commission of Housing and Regional Planning in 1924 estimated that in New York City each person with a separate income supported 1.08 dependents and that the annual income of each family was one and a quarter times the individual income of main support.

While the report does not ask us to conclude anything from these statistics except that the old view is untenable, one does not feel safe in concluding even so little as that from the figures cited. The census, be it remembered, includes all families, not merely workingmen's families. What it tells us about the average size of families, therefore, is not necessarily true of workingmen's families. The report would have been more convincing on this point had it cited figures relating only to workingmen's families. As it is, one is inclined to believe from his personal observation that the typical workingman's family is larger than the average family of the census, but he does not know how much larger.

Again, in stating that the 1920 census "shows that in this average family of

4.4 persons there was an average of two wage earners," the report tends to create a false impression. What the census actually does show is that there are approximately two "persons engaged in gainful occupations" per census family. The census term "persons engaged in gainful occupations," however, is not synonymous with the accepted meaning of "wage earner." A professional man, a man in business for himself, and a man who works for a salary are persons in gainful occupations, but would not be regarded as "wage earners." The National Industrial Conference Board would have inspired greater confidence in its conclusions if it had cited figures for workingmen's families instead of using the census figures for all families and substituting the term "wage earner" for the census term "persons engaged in gainful occupations."

Even if one could accept the statement that in the "average family of 4.4 persons there was an average of two wage earners," one cannot help regretting that the report did not consider it worth while to mention that the earnings of the second wage earner might not always be available for the needs of the family. If he is an unmarried man—and many wage earners as well as other persons engaged in gainful occupations are—he may not contribute to the support of any family. An unmarried woman, too, might not always share her earnings with a family. In so far as wage earners fail to contribute to the support of family groups, there is but cold comfort for the head of a workingman's family in the statistical average given in this report.

So much for the standard of living. How did the investigators proceed to ascertain its cost? To be more accurate, one should say *standards* and *their* cost, for the report presents figures for a variety of types and sizes of families and for individuals. One is impressed with the extent of the price survey. In order to get a fair picture of costs in so large a city as New York, prices were obtained from a large number of localities in all the boroughs of the city. The localities chosen were those in which lived native-born white persons of German or Irish parentage. This was done with a view to getting figures that represent the American standard of living rather than that of the more recent immigrants. The field work in the collection of information about prices is reported to have covered a period of three months, in the course of which "more than a thousand persons, firms, and organizations were consulted for price data and other facts and figures which would make possible an adequate interpretation." The report not only describes in detail the sources of information consulted and the conditions affecting living costs in different sections of the city, but also presents valuable data in tables.

Living costs are computed separately for each of the five boroughs of New York. While there are no great differences in cost from borough to borough, it appears that the Bronx and Manhattan are more expensive for families than the other boroughs and that Brooklyn is the least expensive. For an industrial worker's family of five persons, consisting of the two parents and three children, the annual cost of living at prices prevailing in the spring of 1926 was computed as follows: Bronx, $1,908.42; Brooklyn, $1,841.70; Manhattan, $1,907.76; Queens, $1,893.76; and Richmond, $1,870.37. For an office worker's family of the same membership, the annual cost was found to be $2,085.42 in Bronx;

$2,084.27 in Brooklyn; $2,156.66 in Manhattan; $2,152.95 in Queens; and $2,203.00 in Richmond. Computations were made also for families with two children and only one child, and for single persons of both sexes.

The report is a valuable contribution to the rapidly expanding literature on living costs. It is not, and does not purport to be, an intensive inquiry into the subject; it is rather an effort to obtain by an extensive survey the information that practical business men of the enlightened class will welcome as a help in understanding the needs of their employees. In this effort it succeeds.

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Mathematical Statistics, by Henry Lewis Rietz. Carus Mathematical Monograph No. 3. Chicago: Open Court Publishing Company. 1927. xi, 181 pp.

As the series in which this book appears and as its own title indicate, Rietz's *Mathematical Statistics* is for those who have a comfortable knowledge of mathematics—a bit of differential and integral calculus, of analytical geometry including determinants, and above all the type of mind that can follow an abstract argument. Some proofs are given briefly, some are omitted, there is no attempt to be encyclopedic, none to be practical. Thus the author can hit the high spots, as is appropriate in such a monograph, can indicate some of the most important recent advances in mathematical statistics and point out a few subjects still needing investigation. There is a carefully selected list of references at the end.

The titles of the seven chapters are: I. The Nature and the Problems and Underlying Concepts of Mathematical Statistics; II. Relative Frequencies in Simple Sampling; III. Frequency Functions of One Variable; IV. Correlation; V. On Random Sampling Fluctuations; VI. The Lexis Theory; and VII. A Development of the Gram-Charlier Series. Although, or perhaps because, the interest is chiefly mathematical the author does not fail to point out that the application of probability tables to find the chance that a statistical constant depart by more than a certain multiple of its probable error, and even the determination of that probable error itself, depends on the assumption that the constant in question is distributed for samples of size n according to a normal curve, and that even for tolerably large samples the constant may in fact not be normally distributed.

E. B. WILSON

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Deutschlands Versorgung mit Nahrungs und Futtermitteln, by R. Kuczynski. Parts I, II and III. Berlin: Julius Springer. 1926. 729 pp.

Dr. Kuczynski's study of Germany's provision with food and fodder materials is one of a series of studies of the German food situation prepared by the German Ministry for Food and Agriculture in coöperation with the Reich's Committee for Food Investigation. The first volume is the result of the joint efforts

of Dr. Robert Kuczynski, and Dr. Peter Quante of the Prussian Bureau of Agricultural Statistics. It describes the statistics, official and unofficial, which are available on the subject of the German food supply. The second volume deals with plant food and fodder, giving statistics on areas of cultivation, size of crops, foreign trade and home consumption of the more important foodstuffs before, during and after the War. The third volume deals with animal food and fodder materials in much the same way. The fourth volume which was to have appeared in January, 1927, is announced as dealing with the "Food and Fodder Balance." It has unfortunately not been available to the reviewer.

The introduction to Volume I states that the task of the book is to describe Germany's food supply and its money value. Apparently it is the fourth volume which devotes itself to the money problems, and particularly to the foreign exchange problems, involved in supplying the German population with its daily bread, for money figures do not occur in the first three volumes. In these first volumes, however, the author constantly employs another sort of evaluation; he speaks of the consumption of food before and after the War not only in terms of tons, and grams, but also in terms of protein, fat, carbohydrates and calories. German economists must have learned during the War the hard lesson that food is not measured by its weight alone.

Herr Kucyznski's work shows that the economics of consumption may well add to its tools the measurements of bio-chemistry. To say that the average daily per capita food consumption of a country has declined from 1,983 grams to 1,705 grams would not mean anything in regard to the welfare of the country. It might be good or bad from the point of view of health. To say that the average per capita consumption of protein per day has fallen from 81 grams to 67 grams may mean a decrease in the average physical vitality in the country. Authorities differ on the most desirable amount for protein consumption, but such figures at least supply definite material for discussion, if the ages and occupations of the population are known.

After weighing the statistics showing the home production, animal consumption, industrial uses, waste, and foreign trade for each group of foods, the author estimates the average annual human consumption for each group for two five-year periods—1909 through 1913, and 1920 through 1924. He then calculates the per capita consumption in each period, using a population of 65,333,000 persons for the pre-war period, and 61,500,000 for the post-war period. After the per capita consumption per day has been stated in either grams or liters for each food or group of foods, the protein, fat, carbohydrates and calorie content per capita per day is also given for each. Although there is no table in the first three volumes showing the consumption of all the foods considered, there seems to be no reason for not putting the per capita per day figures together in the table on the following page.

The material in this table presents some interesting and important facts about the reduction in the amounts of food consumed by the German population, but it is difficult to interpret.

The daily energy requirement of the adult male at moderate work is 3,400 calories; and the protein requirement of the adult varies from 75 to 100 grams

GERMAN FOOD CONSUMPTION BEFORE AND AFTER THE WAR

	Average per capita per day 1909–13					Average per capita per day 1920–24				
	Grams	Protein grams	Fat grams	Carbohydrates grams	Calories	Grams	Protein grams	Fat grams	Carbohydrates grams	Calories
Grain products	3,640[1]	28.4	3.5	251.4	1,179	366[2]	28.1	3.7	252.5	1,185.0
Legumes	18.0[3]	3.0	.1	8.2	47	10[4]	1.8	.1	4.8	28.0
Sugar	48.0					45				
Potatoes	500.0	9.3	.9	142.2	621	458	9.0	.8	137.3	600.0
Turnips	40.0 [5]					82 [6]				
Other root vegetables	196.0					168				
Vegetable oils	7.2[7]	....	6.9	.....	64	7.6[8]	....	7.3	....	68.0
Fruit	123.0[9]	.8	.8	15.3	74	115.0[10]	.7	.8	13.8	67.0
Honey	.7[11]	....	....	....	2	.5[12]	....	....	....	1.5
Cocoa and chocolate	2.0[13]	.1	.9	.4	10	3.3[14]	.2	1.5	.7	18.0
All meats	138.0[15]	16.6	49.9	.1	553	89.0[16]	10.8	32.5	....	347.0
Fish	21.0[17]	1.9	1.1	.1	18	21.0[18]	1.9	1.1	.1	18.0
Whole milk	348.0					172.0				
Skim milk	70.0					69.0				
Butter milk	10.0 [19]	19.4	31.8	23.3	470	10.0 [20]	13.4	18.2	15.1	285.0
Goat's milk	52.0					60.0				
Butter	18.0					10.0				
Cheese	12.0					10.0				
Eggs	15.0[21]	1.6	1.5	.1	21	9.0[22]	1.0	.9	.1	13.0
Total	1,982.9	81.1	97.4	441.1	3,039	1,705.4	66.9	66.9	424.4	2,630.5
	Liters									
Wine	.011[23]	....	....	.3	7	.010[24]	....	....	.2	6.0
Beer	.278[25]	.8	....	11.1	111	.137[26]	.3	.....	4.4	44.0
Brandy in terms of alcoholic content	.008[27]	....	....	....	43	.0025[28]	....	....	....	14.0
Grand total		81.9	97.4	452.5	3,200		67.2	66.9	429.0	2,696.5

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13	II	378
14	II	381
15	III	58
16	III	64
17	III	96
18	III	98
19	III	124
20	III	135
21	III	145
22	III	146
23	II	392
24	II	393
25	II	397
26	II	400
	(Post-war beer is estimated to be 20 per cent less nutritious than pre-war beer)	
27	II	402
28	II	403

per day. The only way to interpret the consequences for health of this decline in per capita consumption would be to calculate the average calorie and protein requirements of the German population from a table showing the age and occupation composition of that population. It may be that Herr Kuczynski makes such a calculation in his concluding volume, and if he does it will add greatly to the understanding of the German food situation. Further figures are particularly necessary because of the probable change in the composition of the German population during the war period. It seems likely that children under fourteen years of age formed a larger proportion of the whole from 1920 through 1924

than from 1909 through 1913. The proportion of children in the group is especially important in the measurement of food consumption since children require more calcium and phosphorus than adults and less protein, calories and iron.

The decline in milk consumption is the most striking fact shown in the above table, and it is unfortunate that the author, having gone so far in analyzing the food consumed and having given its protein, fat, carbohydrate and calorie content, could not have gone farther. The amounts of iron, of calcium, of phosphorus and of vitamins available to a given group are quite as important as the protein, or the calories. German children have probably suffered as much, if not more, from a lack of calcium and of foods providing the anti-rachitic vitamin than from a lack of protein, important as a proper supply of protein is in promoting child growth. The amounts of iron, calcium and phosphorus consumed in Germany before and after the War could have been calculated from Herr Kuczynski's food consumption figures without much extra work. Estimates of vitamin content are less easy to make, and the only method I know of indicating changes in the vitamin consumption would have been to have isolated the foods which have been found to have a high vitamin content and to have compared their pre-war and post-war consumption in grams.

It is impossible in the space of a review to indicate the methods by which the estimates of food consumption have been reached. They are painstaking and detailed. Since the decline in milk consumption is so great (from about a third of a quart per day before the War to about a sixth of a quart after the War), it may be worth while to review the method by which those estimates were computed.

Statistics in regard to the number of domestic animals in Germany were first collected in January, 1873, and after that at irregular intervals. The last general census before the War occurred in February, 1912, and in that census and in the 1907 census milk cows were separated from other cattle. Prussia and several other German states made annual surveys in the years before the War, so that the estimate of the average number of milk cows in Germany from 1909 through 1913 rests on a good foundation. Since the food problem has grown so acute in Germany, figures on the number of cows in the country have been collected annually (in 1918, 1919, and 1920 quarterly) so that the post-war figures are even more accurate than the pre-war figures. The question of the yield of milk per cow before the War was more difficult to settle. Official estimates were available from Bayern and Württemberg only, but pre-war unofficial estimates were abundant. The estimate chosen was 2,200 liters per year. In 1916 the Reichs Bureau for Food Fats found that the yield per cow was 1,800 liters, in 1917, 1,500 liters, and by 1918, 1,300 liters. Herr Kucyznski estimates that the average annual yield 1920–1924 was 1,500 liters. The reason for the great decline in the yield of milk per cow after the beginning of the War was the dependence of the German dairy industry upon imported oil cake and grains. The author estimates that 70 per cent of the milk products consumed before the War were dependent on imported materials.

The book presents a storehouse of information in regard to German agricultural

production and foreign trade in food, and the fourth volume will undoubtedly add greatly to an understanding of the reparations problem.

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Profits, Progress and Prosperity, by Arthur B. Adams. New York: McGraw-Hill. 1927. ix, 178 pp.

This volume is apparently an expansion of an unfavorable review of Foster and Catchings' *Profits*. But the author has not been content to make a negative attack on the Foster and Catchings theory; he has attempted to replace it with a constructive theory of his own. In the present reviewer's judgment he has been more successful in the former than in the latter task.

The principal point which Professor Adams makes against the Foster and Catchings theory may be summarized as follows: Savings which are invested in permanent capital equipment do not cause a subsequent shortage of consumer income, under conditions of a stable price level. If the expansion of capital equipment causes an increase of production of consumers' goods, it will cause a corresponding increase in the national money income. The new capital is used just as the old capital is used, and in the process, wages, interest, and rents are created to balance the new supply of consumers' goods. Professor Adams agrees with Messrs. Foster and Catchings, of course, that to maintain prices there must be some expansion in the circulating medium from year to year as the volume of money work increases, but he is more apprehensive of our getting too much than too little expansion. He also shows statistically that the amount of increased production which can be attributed to the capital equipment created through saving in any year is much too small to give rise to a boom.

Professor Adams' own theory, in brief, is that depressions are caused in one of two ways. Minor depressions are caused by the accumulation of excessive stocks of circulating capital goods financed out of past and current savings. The attempt to liquidate these stocks results in a temporary excess of consumers' goods over consumers' current income, which causes declining prices, declining profits, and curtailment of production. Major business depressions are caused in the same way, except that the accumulations of circulating capital goods are financed not only by current and past savings but also by an expansion of bank credit. This causes a boom and a larger accumulation of circulating capital goods than will arise under "non-boom conditions." The liquidation process following such a boom is characterized, as is a minor depression, by an excess of consumers' goods over consumers' purchasing power, but that condition is accentuated by the pressure of liquidation of bank credit. A boom is brought to a close by one or all of three conditions: the exhaustion of the supply of bank credit, the rise of costs above selling prices, and the final appearance of a supply of consumers' goods in excess of consumers' current incomes. On the latter point the argument (pages 161–2) seems to contradict the argument against Foster and Catchings' theory of a shortage of consumer income (pages 39–41). The one discussion re-

fers to a condition of stable prices, the other to boom conditions, but allowance for this difference does not, in the reviewer's judgment, remove the contradiction.

The volume is of little interest to statisticians, unless they are also students of the theory of business cycles. Most of the argument is based on logical deduction rather than historical experience; and when appeal is made to the facts, the latter are seldom presented in statistical form. To be sure, there are two chapters which discuss selected statistics relative to the amount and distribution of the national income, but few of these data are used to support the author's conclusions. Reference has already been made to one item of considerable interest, a calculation of the proportion of national income which is saved in the form of equipment for further production, and the possible consequent expansion of output of consumers' goods.

There is a tendency to omit the supporting evidence or argument for generalizations which are not self-evident. For example: "It takes relatively more capital investment in proportion to the value of the output to produce finished products when the general price level is rising" (page 160); "Consumers gain very little, if any, in volume of goods consumed during boom conditions" (page 172). There is also a tendency toward exaggeration. For example: "During major depression periods there is . . . little or no demand for additional equipment" (page 127); "Practically all business men become sellers and few of them buyers" (page 165).

The style is very faulty. Verbs and nouns frequently disagree, and incoherencies are frequent. For example: "According to the extent to which capital is substituted for labor the owners of the capital will get all the returns from the employment of the substitute" (page 96); "When the supply of additional laborers is scarce" (page 96); "Such loans are rarely ever made" (page 69); "More than 50 per cent of the income receivers are found to have received less than $1,200 each and that they received less than 30 per cent of the total income received by individuals" (page 104); "A comparative stable price level" (page 114); "There is a tendency for laborers to lay down on the job" (page 160).

Professor Adams' book is worthy of careful study. His emphasis on the importance of speculative accumulation as a cause of price fluctuations brings out a factor which, though not new, has probably been under-emphasized. By his criticism of Foster and Catchings' doctrine of the relationship between capital accumulation and consumers' income he deals that much-battered volume a blow at one of its vital spots. His doctrine of the independent causation of each new upward swing—independent, that is, of the effects of the preceding cycle—is worthy of careful study. He has not solved the problem of business cycles, but he has made an interesting contribution to the list of suggestive but unverified hypotheses concerning the nature and causes of these phenomena. It is to be regretted that his conclusions are presented as theses rather than hypotheses.

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Banking Policy and the Price Level, by D. H. Robertson. London: P. S. King & Son, Ltd. 1926. 103 pp.

The advisability of price stabilization has become a commonplace in economic discussion in recent years. At the present time there is pending in Congress a bill which would make it the duty of the Federal Reserve system deliberately to direct its policy towards stabilization as far as lies within its power. It is, therefore, a matter of more than academic interest to find a leading English economist entering a demurrer as to the adequacy of a policy of stabilization. Such is the position taken by Mr. Robertson in his essay on *Banking Policy and the Price Level*.

The essay is highly abstract and theoretical. There has been practically no attempt to introduce supporting factual material and the analysis itself has been cut to the bone so that there remains, as the author says, but a theoretical skeleton; a bare outline as it were, unadorned by any literary surplusage and relieved and clarified by a minimum of concrete illustration. As a result the reading of the essay involves a severe mental discipline. In the introduction the author warns the reader that the book is difficult and advises that each chapter be read a second time before the next one is started. In all probability no reader who gets beyond the first chapter will be inclined to question the soundness of this counsel.

In brief, the argument of the essay is that there are certain economically justifiable changes over a period of time in the volume of industrial output, that the banking policy of a country should be so adjusted as to bring about these changes most effectively, but that the achievement of this end is incompatible with an undeviating policy of price stabilization.

The first concern of the author is to show that there are rational incentives to alterations in the volume of production; that we should not expect the appropriate or optimum rate of industrial output to be constant but to be subject to a succession of what may be called "justifiable" increases and decreases, some at least of which are of a fairly rythmical nature. In order to demonstrate that such justifiable changes in output are not peculiar to our money economy with its privately operated industries, Mr. Robertson first assumes a society where exchange is effected by means of barter, and production is carried on by coöperative groups somewhat along the lines advocated by guild socialists. Under these conditions there would be various rational incentives to any group of producers to alter its scale of output. For instance, if one group of producers develops a more intense desire for the products of another group, it would be reasonable for the first group to increase its output in order to get by exchange more of the goods that it wants. Such an increased desire might, for example, come about from inventions leading to a demand for the implements or materials involved.

In this part of the analysis we are transported to an atmosphere of unreality inhabited by beings whose motives the producers in this world in which we actually live would have difficulty in comprehending. We are to understand, for example, that the desire of a group of cotton spinners for a new kind of machinery would lead them to work harder and turn out more cotton yarn so that, it seems,

both the machinery and other things to which the group had been accustomed might be obtained in exchange. While it is possible that changes in production might be motivated in this way in the imaginary world to which Mr. Robertson introduces us, the analysis appears to have little relation to the economic world with which we are familiar, where productive operations are in charge of business men dominated by the motive of money profits.

But we are now told by the author that the analysis is to be brought into closer accord with real life by gradually taking into account some of the actual features of our wage and monetary system. This further analysis, however, is merely an attempt to show that under employers, actuated by the same motives as those previously considered, the fluctuations in output would be even greater than under the hypothetical coöperative groups; and in this portion of the study money is introduced only to the extent of examining what change in the monetary supply of a country would be most effective in bringing about a change in production corresponding to the fluctuation that would have occurred in the imaginary barter society previously studied. It is decidedly questionable, however, whether what is looked upon as an appropriate fluctuation in production in a barter society actuated by the motives which the author has studied is likewise the appropriate fluctuation in a money society where the predominant productive motive is money profit. No economist would desire absolute constancy in the rate of industrial output nor doubtless does any one believe that uniform progression in the rate is a possibility. All would probably agree that there will always be some fluctuations in output. But Mr. Robertson's analysis does little in helping us to understand the justifiable extent of these fluctuations in the actual economic world of today.

In this part of the study the conclusion is reached that a policy aiming at ultimate stability of the price level is not the most effective policy for the monetary authority to adopt. Thus, if, according to the underlying conditions of demand, an increase in the output of certain goods becomes "appropriate," such an increase, it is said, may be most effectively brought about by injecting a quantity of money into the system that would cause an increase in the general level of prices. In the following chapter, however, it is admitted that the increased prices may lead to an enhancement of production that is more than appropriate. But just how the monetary or banking authorities are to determine the proper rise in prices is not indicated.

Mr. Robertson here finds it necessary to interrupt the development of his main thesis in order to undertake an inquiry into the nature of capital and of the activity by means of which capital is provided. This activity is called "Lacking" rather than by the more usual term "Waiting." A man is said to be lacking during a given period if he consumes less than the value of his "current economic output," but the latter term is not defined. Fixed and circulating capital are differentiated and the distinction is made between long and short lacking—that is, lacking for a long time or a short time—which are the correlative activities toward the provision of fixed and circulating capital respectively. In this chapter a whole vocabulary of unfamiliar terminology is introduced to the reader. It is irritating at the first reading, but a more careful study makes it apparent that

it has served the purpose of enabling the author to express his ideas within a much briefer compass. It is pointed out that the lacking necessary for the provision of capital is not always the result of voluntary saving on the part of individuals or corporations but is sometimes imposed on the public through the banking system by means of raising prices through an increase in the circulating medium. Apparently through this process certain people whose incomes do not increase as fast as prices find themselves compelled to consume less, but inasmuch as their output remains the same, the amount of lacking—the difference between consumption and current output—is increased. In this chapter the author has very carefully worked out a suggestive analysis of an extremely intricate problem —the relation between the banking system and the accumulation of capital. An attempt is made to trace the internal mechanics of a process of inflation in an appendix to the chapter which will probably be clear only to the mathematically inclined reader.

To get on with the main theme, let us assume with the author that there are certain justifiable fluctuations in industrial output. These will in turn necessitate changes in the supply of circulating capital and of short lacking. Thus, in the upward swing of the trade cycle there is a large and discontinuous increase in the demand for short lacking. The supply of voluntary short lacking is not sufficiently elastic to cope with this demand and the responsibility for meeting it rests almost entirely upon the banking system. This function is accomplished, however, by extracting the necessary short lacking from the public through an increase of prices effected by means of inflation. Consequently it "seems unreasonable to expect the banking system *both* to ensure that appropriate additions are made to the quantity of Circulating Capital *and* to preserve absolute stability in the price level." It is recognized, however, that various factors may lead to a greater increase in the price level than is necessary for securing a proper adjustment of short lacking. It is the duty of the banking system to check such secondary increases in prices, by the familiar methods of raising discount rates, sale of government securities, and direct limitation of loans. Unfortunately no clues are given to indices that would guide the practical banker as to when the reasonable increases in prices ended and the unreasonable ones began.

In the last chapter, dealing with the relations of short and long lacking, it is difficult indeed to follow the intricacies of the author's analysis. We are first informed that in the upward movement of the trade cycle, the increase in the rate of output of capital instruments requires, for the preservation of equilibrium, a sustained increase in the stream of long lacking for embodiment therein. Therefore it would not be surprising if voluntary supplies of long lacking—that is, long lacking provided by voluntary investments—should prove unequal to the strain imposed on them, so that a call is made upon the banking system to provide increased supplies of long lacking. The banking system, however, can provide long lacking only in the same way as it provides short lacking, namely, by extorting it from the public through multiplication of the currency, thereby causing an increase in the price level. Thus once more the rise in prices may be a rational and justifiable one. However, the strain placed upon the banking

system by the necessity of providing this long lacking as well as the short lacking, as previously explained, is likely to precipitate a crisis. The author thinks that the crisis thus precipitated would come sooner than if it had been dictated by the stage reached in the true constructional cycle, that is, by a revaluation as to the desirability of acquiring instruments. However, it is said, the moment for such a revaluation would soon have arrived at any rate.

Proceeding with the analysis of the trade cycle, Mr. Robertson says next that we must take account of the complementary nature of long and short lacking and of the obligation which rests upon the banking system to preserve some sort of balance between them. Although he has just explained that in the upward swing of the cycle it would not be surprising if voluntary supplies of long lacking should prove unequal to the strain imposed on them, on the other hand, we are now told that the pace is set by what appears to be voluntary long lacking—in the words of the author, "by investors, with their increased offers of Long Lacking." Therefore in order to preserve a balance between the two kinds of lacking, the banking system must provide coöperant short lacking with the usual result of an increase in general prices. Perhaps the author has been misunderstood, but we seem to get nowhere with this part of the analysis.

One other matter in this connection remains to be noted. In the previous chapter it had been pointed out that those theorists were right who have found the cause of crisis in a "deficiency of capital" but not in the sense that there was necessarily a deficiency of goods, since the available information seems to show that at such times the stocks of goods may be both large and even on the increase. "What is deficient is the *activity*, essential to the production and storage of goods, which we have described as Short Lacking." Now we are again told that it is an error to identify the "shortage of capital" which precipitates a crisis of the constructional type with a shortage of stocks of consumable goods. "But," Mr. Robertson says, "it is possible, nevertheless, that the *rate of output* of consumable goods should be so small *as compared with that of constructional goods*, as to embarrass the progress of the constructional boom both directly by enhancing the cost of production of instruments and indirectly by enhancing the probable costs of their operation and therefore diminishing the advantage of acquiring them." This would appear to be merely another way of saying that there is a shortage of consumable goods—a shortage as compared with the output of constructional goods—and is thus hard to reconcile with the previous statement. Be that as it may, it is urged that in this way there would be a decrease in the desirability of acquiring instruments because of the "lack of responsiveness in the flow of consumable goods required to coöperate with those instruments in the form of real wages." It is not clear from the author's account just how this works out in our money economy. Perhaps we are to assume that the modus operandi is as follows: The failure of consumable goods to increase rapidly enough leads to an increase in their price; the consequent increase in the cost of living leads to an increase of wages, which increases the costs of instruments and cuts down the profits of employers who therefore purchase fewer instruments. This analysis, however, assumes an unwarranted closeness in the correspondence between money wages and the cost of living and takes no account of the prices at which

the employer can sell his products, which would seem to be the important factor guiding him as to the desirability of acquiring more instruments.

Probably the most enlightening general criticism that might be made of this study is one from the author himself in the form of a delicious quotation on the title-page from *Through the Looking Glass:*

> "She's in that state of mind," said the White Queen, "that she wants to deny *something*—only she doesn't know what to deny!"
> "A nasty, vicious temper," the Red Queen remarked.

We can certainly absolve the author from the imputation of a vicious temper, for the entire essay is in an altogether dispassionate style. There does, however, appear to be some uncertainty as to what is to be denied. It seemed at first as if it were to be the desirability of the policy of price stabilization. But he says that he is in favor of a nearer approach to price stability than we have had in the past; and although he contends that justifiable fluctuations in industrial output may necessitate changes in the price level, he admits that such changes may be carried too far without, however, indicating any practicable guide as to where they should cease. He closes with the thoroughly sound but perhaps equally futile conclusion that it would almost seem that to act beneficially at every stage of the trade cycle bankers would have to become infinitely wise as well as infinitely virtuous.

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Negro Illegitimacy in New York City, by Ruth Reed. New York: Columbia University Press. 1926. 136 pp.

The consideration of the Negro problem has been characterized in the main by the employment of large measure of hearsay and opinion rather than factual material resulting from actual research. Nowhere is this more true than in those facets of the problem which have to do with the sex life of the Negro. Official statistics of birth and death are not easy to obtain, while the current opinion of the sexual looseness of the Negro—something which has come to be associated with his biological make-up and regarded as a racial characteristic—is reiterated so often as to be considered accepted and proved fact.

This situation, of course, is one which will be cleared up with the passage of time and the realization that the Negro problem, like all others, may be attacked in a scientific manner and only intelligently understood when the facts are available. And we are indebted to Dr. Reed for her contribution to this process. Her study of Negro illegitimacy is one which may well serve as a model for students who are interested in the difficult problem of the sex life of the Negro in its sociological aspects. In her little volume we find no tendency to go beyond her data, to bring in extraneous "racial" factors, or to make comparisons which should not be made. It is a sociological study, and she keeps it such. Her book

> does not aim to settle the question of how illegitimacy among Negroes differs from illegitimacy among white people. The object is merely to present descrip-

> tive material with regard to some of the problems created by illegitimacy among a selected group of Negroes in New York City with the hope that such material may prove useful when comparable data for white groups shall be available.

In a word, her study represents the kind of ground-breaking which must come before many of our assumptions as to the sex life of the Negro may be regarded as tenable.

Her data consist of 500 cases of unmarried mothers which she obtained from various social work agencies in New York City. While she recognizes the methodological difficulties which stand in the way of the use of material of this sort, she utilizes her facts with caution so that the results derived from them are valid. Dr. Reed does not claim that her sample is representative of all the Negroes of New York City who are unmarried mothers, let alone the entire country. Knowledge of the "lay of the land," greater maturity, and advantageous economic position on the part of the women who bear illegitimate children, we are told, all go to help them avoid resorting to social agencies for assistance. Dr. Reed's sample consists of those who received assistance, and to that extent are a selected group.

Starting with a discussion of the position of the mother who bears her child outside the recognized form of wedlock, she shows that while social control is to be found in all societies, the force of the control varies with the pattern of the group and the cultural mold in which the offense is set. She then passes to the women of her sample. With the locale of her study New York City, she finds that most of her 500 cases were born outside the city; that the ages of these women ranged from 12 to 41, the great majority being in the decade 16–26; that comparatively few had finished the eighth grade of school; that the religion of the girl's family often affected her situation when her pregnancy was discovered; while 85 per cent of the cases were women who were employed in domestic service (an occupation where the rate of illegitimacy is notoriously high). The economic and social difficulties which face the unmarried mother are next considered—where she lived when she became pregnant, her relation with the father of the child after its birth, and the age and economic status of some of the fathers.

Before reaching her conclusions, Dr. Reed takes up the important problem of the social readjustments of these women after the birth of their children. There is an extremely interesting section in this chapter on the attitude of the mothers toward their children. In view of the prevalent hypothesis of the maternal instinct, it is striking to note than in 94 out of 463 cases the attitude remarked was one of indifference or hostility toward the child. Of course, this may have a social basis, but after reading Dr. Reed's discussion, one is not so sure that the positive side of the question, the existence of strong affection for the child, is not a matter of the absence of social disapprobation, in appreciable measure. To quote,

> Our data lead us to the suggestion that the attitude of the mother toward her child is correlated to a high degree with the hardship she is forced to undergo as the result of the child's existence.

In her summary, Dr. Reed discusses in illuminating fashion the manner in which Negro *mores* are akin to those of the larger white population of which the

Negroes form a part. She remarks the accentuation of those aspects of our culture which are generally approved, and shows how the aspect of "sexual uprightness" has been stressed among certain Negro groups.

> By this group the suggestion that Negroes might conceivably develop a system of sex morality and family life different from that of the other elements of the community is indignantly resented.

And in ending, she concludes that while there are higher rates of illegitimacy among Negroes than other population classes,

> there is no conclusive evidence that Negro women are meeting their problems of family life in any way which may be regarded as characteristically racial. . . . In order to isolate differences in illegitimacy problems due to race we would need to compare the rates for different racial groups under substantially the same conditions in this country and data for these comparisons are not available.

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The Widening Retail Market and Consumers' Buying Habits, published for the Bureau of Business Research, Horace Secrist, Director, Northwestern University. Chicago: A. W. Shaw and Company. 1926. viii, 186 pp.

For almost a decade, now, the problem of the physical distribution of economic goods has been occupying the attention of a growing number of economists. The tremendous growth of machine production, the increase of standardized manufacture, and the elimination of waste in the factory and mine have raised new problems for the distributor—problems which will have to be solved before the consumer will be able fully to enjoy the savings in production costs which have been made possible by "the new industrial revolution." Instances where savings in production costs are offset by increased distribution charges are still far too common in modern industry.

The most desirable method of attacking the problem of wasteful distribution still remains to be decided upon. Until we know the facts of the case little can be done. It is to be hoped that the results of the elaborate and painstaking census of the distributive process now being undertaken by the United States Department of Commerce will answer in some measure the question as to how considerable is the unnecessary burden which is now borne by the consumers of commodities sold in the retail market.

The study of *The Widening Retail Market and Consumers' Buying Habits* does not, as evidenced from the title, concern itself with the vital question of distributive costs. It deals rather with the process whereby modern advertising and efficient transportation facilities "serve to link together into a single market the trading centers for men's clothing and furnishings" (page 5). It is the authors' thesis that the retail market for men's clothing and furnishings is a " 'connected system' of purchase and sale co-extensive with the distribution of population in the United States" (page 38). They arrive at this conclusion by an analysis of the buying habits of 4,643 persons—college students and their fathers. These people, they find, purchase an appreciable amount of clothing and furnishings in cities other than those in which they reside. Purchases, in

other words, overlap, and different trading areas are brought into active competition with each other.

In reaching their conclusions, the authors have uncovered much data which should prove of value to the clothing industry. They have found, for example, that the student living in the small town is more likely than is his father to buy his clothing in cities other than that in which he lives. Their data show, further, that "clothing is more frequently purchased 'abroad' than are furnishings" (page 98). They disclose also the fact that even though trade flows toward the larger cities the ability of such cities to attract custom depends "not so much upon their absolute as upon their relative size and accessibility" (page 99). These facts and the statistical data upon which they are based should be of help to the clothing sales manager in planning his advertising campaigns. The Appendices contain by geographical districts the names, location, and population of the trading centers concerning which information has been secured as well as "the places of residence of consumers whose customary practice it is to buy in places other than those in which they live."

The volume is an interesting excursion into the question of markets and should prove of considerable interest to the many economists who because of their concern with price theory have been compelled to speculate upon the nature of markets.

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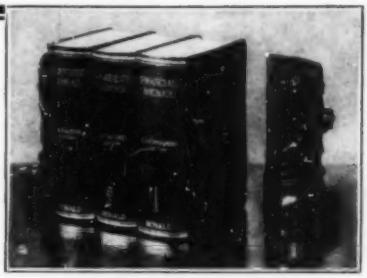

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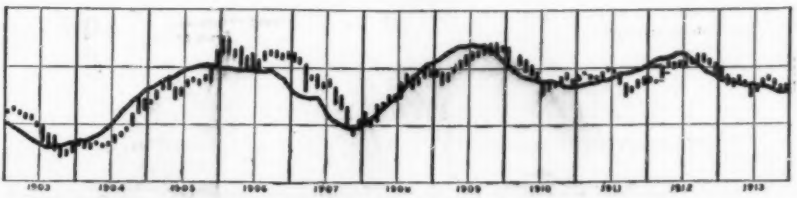

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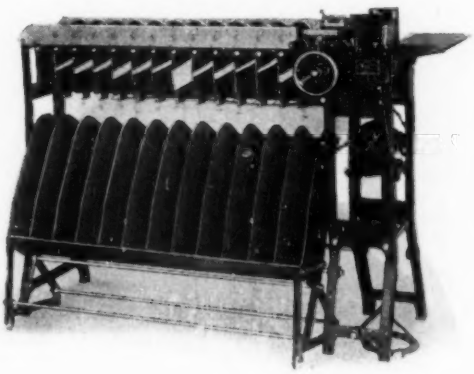

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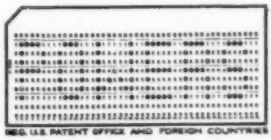

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